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The Contribution of Economic Theory to Economic Prognostication[1]

By R. C. TRESS

My subject is economic prognostication and my reason for choosing it is a simple one. Call it credential or confession as you will, the fact is that, for the last fifteen years or so, I have been a practising economic prognosticator. I attended at the birth of the *Economic Survey* and official attempts at longer-term planning, while the London and Cambridge Economic Service, having elected me a member of their Editorial Board, generously allowed me to remain even after I had ceased to possess a residence qualification.

Prognostication is, however, a modest art, more akin to the topical cartoon than to a master painting. Therefore, it is not my intention, in this lecture, to survey past successes and failures. The failures are obviously best forgotten; and I doubt very much if a recital of the successes would do much to advance me in your estimation. Rather, if I so much as ask, " Who warned you of the crisis of 1951 ? ", or " Who forecast the boom of 1954–5 ? ", you will immediately rush to the conclusion that I am selling tips for the April budget.

On the other hand, I do not intend to launch into any methodological defence of economic forecasting. I am satisfied with the axiom that " economics is what economists do " and that there is a sufficient number of us in the racket to be able to raise a sizeable lobby if ever the attempt should be made to deprive us of our fellowships in the Royal Economic Society. If, however, that test should seem too democratic for some tastes, then I will appeal, alternatively, to holy writ. " Explanation," wrote Alfred Marshall at the beginning of *Industry and Trade*,

> is simply prediction written backwards: and, when fully achieved, it helps towards prediction. A chief purpose of every study of human action should be to suggest the probable outcome of present tendencies; and thus, to indicate, tacitly if not expressly, such modifications of those tendencies, as might further the well-being of mankind.[2]

This quotation from Marshall does more than provide me with a methodological defence, however. It gives me a jumping-off point for the enquiry which I wish, in this lecture, to pursue: if economic theory is forecasting written backwards, what then is the contribution of economic theory to economic forecasting ?

The question is a historical rather than a philosophical one, and I shall confine my pursuit of it to the period which I know best: that of my own professional lifetime (to date). I shall first consider the state of economic prognostication and its relation to economic theory—in particular, national accounting—in the early 1940's: that is, at the beginning of post-war thinking and planning. Secondly, I shall set out what seem to me to be the salient features, from the standpoint of prognostication, in the developments in economics which have taken place in the post-war period up to the present time. Thirdly, I shall enquire how far those developments have affected and benefited economic prognostication as currently practised and exhibited. And finally, I shall suggest points at which more help is needed and where more help might, in fact, be found.

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[1] A public lecture under the auspices of the University of London, delivered at the London School of Economics and Political Science, 5th March, 1959.

[2] A. Marshall, *Industry and Trade*, 1919, p. 7.

I

The magnitude of the technical differences between economic forecasting before and since the war can best be indicated by illustration. Before I offer the illustrations, however, let me emphasize that it is *technical* characteristics which I am contrasting. In another context,[1] Professor Johnson has enjoined us to distinguish between " the development of analytical technique—the rate of technological progress in the machine-tool industry of economics " and " the degree of understanding of economic processes and problems . . . the calibre of the entrepreneurs ". We should avoid identifying inferior or superior technique with inferior or superior economic insight. Such a caution is especially needed at this point because my first quotation comes from a pre-war number of the *Bulletin* of the London and Cambridge Economic Service, and he would be a bold man who would challenge the " entrepreneurship " of the Service's Editorial Board of those days. The statement of " the general business position " of the United Kingdom given in a *Bulletin* for early 1937[2] is typical, and reads as follows:

> The rise in wholesale prices has accelerated and has spread to all groups of commodities. In eighteen months, according to the *Statist* index, prices of materials have increased nearly 20 per cent. The general rise for all commodities in the same period was, according to the Board of Trade, 14 per cent. Meanwhile the Cost of Living Index increased by 5 per cent. There now appears to be some tendency for long-term interest rates to rise, and prices of industrial securities have been oscillating near the highest level yet reached. Short-term rates have returned to their pre-December level.
>
> In some directions the rate of progress is damped down. The December statistics of unemployment show some increase, even after seasonal and weather influences have been taken into account. The Index of Production, though 5 per cent. higher than in the last quarter

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[1] H. G. Johnson, "Demand for Commodities is *not* Demand for Labour", *Economic Journal*, December, 1949, pp. 531–2.

[2] Royal Economic Society, Memorandum No. 65, *Report on Current Economic Conditions, March, 1937;* the statement on " the general business position, United Kingdom " is dated 19th January, 1937.

> of 1935, has hardly shown the usual seasonal rise, and the increase compared with a year ago is less than in previous quarters. A similar slackening has occurred in imports of materials.
>
> On the other hand, when months are averaged to eliminate some unusual fluctuations, exports during the second half of the year have been on a higher level than in 1935; in particular the December figure was higher. With the increased purchasing power in countries selling us raw materials, some further impetus may be expected towards increased exportation during 1937.
>
> The general result of the statistics is to suggest that we are approaching a maximum of activity. There is no indication of a generally unhealthy condition or fear of a crisis, but the rate of progress is slackening.

Now if one compares a statement of this kind with any contemporary statement of like standing—say the present-day statements of " the economic position " in the London and Cambridge Economic Bulletins or (to come right up to date) the " Summary " in the first number of the new *National Institute Economic Review*—one is struck by three marked differences. First, there is in the modern statements a much larger quantitative reference. More figures may not actually be quoted but certainly a wider range of statistical knowledge is implied. This, of course, is the consequence of the immense increase in statistical output by the Government since the war. Secondly, there is a larger policy content in the conclusions. It is not enough to report what is happening or is likely to happen—to say, for example, that " the rate of progress is slackening "—one must go on to indicate what the Government should do about it. And, thirdly, there is a recognisably ordered framework to the analysis of the situation—the framework of national income accounting. No better example of this could be found than in the *National Institute Economic Review* on the " prospect for 1959 ":[1]

> If no further Government measures were introduced demand would, in our view, recover gradually, but the change would be slight. Neither incomes nor prices are expected to rise much: consequently, though consumers' expenditure on durables should continue to be higher, purchases of other goods and services may rise little. A fall in private investment is expected; this should be offset by the rise in public investment and in Government current expenditure. Exports may rise gradually and the fall in stocks may soon be reversed.
>
> These small changes would be unlikely to raise output much above the 1957 level. Employment would probably rise less than production. Imports would probably rise more than exports, and bring the current balance of payments surplus back to about £200–£300 million.

Pre-war, it may be said, the statistical data relating to the progress of the national economy—or such as were available—were put through a process which appears to have been almost wholly intuitive. Post-war, there is not only a far larger supply of statistics; they are put through an identifiable machine.

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[1] *National Institute Economic Review*, Number 1, January 1959, p. 3.

What, however, is the nature of this machine and what is its connexion with economic theory? The comparison which I made just now related to 1937 and 1959, two years when there was more or less a free economy. It is well to remember, however, that the national income framework for the analysis of economic events was introduced during the war and was brought into full and public use in the late 1940's, the peacetime years of restrictions and controls. National income accounting provided a technique for planning: that is, for considering each of the claims on the national output in turn and for devising means whereby their permitted amounts should add up to no more than the available total. It was in terms akin to these that Mr. E. F. Jackson described the British use of the technique to the International Association for Research in Income and Wealth in 1949.[1]

Now, for the purposes of planning of the post-war kind (" semi-planning " as Mr. Jackson called it) the national budgeting model made up of accounting relationships between the components of final demand was reasonably adequate. It is true that Mr. Kaldor, when he deployed the technique on behalf of Lord Beveridge in 1944,[2] included certain behaviouristic equations in addition. Consumption itself, direct and indirect tax liabilities, were all included in his model as linear functions of income. But he was positing full employment " in a free society ". In the actuality of the late 1940's, whilst undoubtedly the size of personal incomes was of real significance for consumption demand—and was, indeed, one of the main issues between the Government and its critics—the continued operation of rationing and controls and still more the existence of large accumulations of wartime savings made the inclusion of a simple functional relationship between income and consumption impracticable. A technological input-output analysis to back the accounting relationships of final demand would have been instructive, but it did not exist.

My reference to the very latest line in economic forecasting in this country, the *National Institute Economic Review*, is sufficient to show that the national income framework which forecasting adopted in the planning and control days of the 1940's has by no means become obsolete in the freer and more fluctuating economy of 1959. The most significant result of greater freedom is that the level of activity of the economy is dominated by the forces of demand, and the national income " model ", being an exhaustive classification of the components of demand and of their accounting relationships, obviously fits the later situation. It is, in fact, an invaluable machine for organizing and checking the thinking of contemporary economic forecasters. The individual economist who contemplates current events and statistics has a statistical frame of reference by which to gauge his orders of magnitude and a check-list to ensure that he has allowed for the whole situation and not just some part of it. Furthermore, for a group of economists, coming together to discuss " the economic position ", the model provides not only these same controls but also a ready-made and readily appreciated agenda.

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[1] E. F. Jackson, " The Recent Use of Social Accounting in the United Kingdom ", in International Association for Research in Income and Wealth, *Income and Wealth*, Series I (ed. Lundberg), 1951, pp. 155–7.

[2] N. Kaldor, " The Quantitative Aspects of the Full Employment Problem in Britain ", Appendix C to W. H. Beveridge, *Full Employment in a Free Society*, 1944. See also J. R. N. Stone and E. F. Jackson, " Economic Models with Special Reference to Mr. Kaldor's System ", *Economic Journal*, December 1946, pp. 558–563.

When one has said all this, however, one has only said that national accounting is an enormously valuable device for organizing calculation, thinking and discussion. It is not an analysis of economic behaviour and it is with behaviour, present and future, that economic forecasting is concerned. The simple national budgeting model involving only accounting relationships gets us so far but, to quote Professor Stone, " if estimates of the future values of the variables which it contains are to be narrowed down, information about behaviour or technology must be introduced ".[1]

Now, the national accounting system based on final demand has its origin in Keynes's *General Theory of Employment, Interest and Money* and in the subsequent adaptation of that theory to the problems of war finance. The natural place to look for further technical improvements in economic forecasting, therefore, is in the behaviouristic and technological components of the *General Theory* and in the developments which have taken place in macroeconomic theory since Keynes.

Of course, you may decide that the best statistic by which to conduct our economic affairs is an index of the aggregate blood-pressure of the Zurich bankers (seasonally *un*corrected, of course). You may insist, as Dr. Clark Warburton has done with respect to the 1930's,[2] that the most potent factor of economic instability is not the principal Keynesian variables but erratic variation in the quantity of money. Or, encouraged perhaps by the attention which Professor Patinkin gives to it in his theoretical studies,[3] you may assign the major role in economic affairs to the effective working of the interest mechanism. But if you will accept that, in Professor Hicks's words, " we get a better understanding of these problems by considering effects on employment and income first ",[4] then the national budgeting model, founded on the Keynesian general theory, is the proper analytical foundation for economic prognostication. We have then to see what livelier engines have lately been added to it and how, possibly, we may use them.

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[1] R. Stone, " Model Building and the Social Accounts: a Survey ", in International Association for Research in Income and Wealth, *Income and Wealth*, Series IV (eds. Gilbert and Stone), 1955, p. 29.

[2] C. Warburton, " The Misplaced Emphasis in Contemporary Business-fluctuation Theory ", *Journal of Business of the University of Chicago*, reprinted in American Economic Association: *Readings in Monetary Theory*, 1951, pp. 284–318.

[3] D. Patinkin, *Money, Interest and Prices: an Integration of Monetary and Value Theory*, 1956.

[4] J. R. Hicks, " A Rehabilitation of ' Classical ' Economics ? ", *Economic Journal*, June 1958, p. 288.

II

Any attempted summing up of post-Keynesian macroeconomic theory into a single, unified whole would go beyond the facts. No such unified system exists. There are, however, three general features in the large amount of macroeconomic enquiry undertaken since the war which I think are worth commenting upon as being of particular potential significance for the work of economic forecasting. I propose to look at each of these three in turn.

First and foremost, there has been established in broad—very broad—terms a consensus regarding the essential constituents of a satisfactory macroeconomic theory. This consensus can be summed up briefly into four propositions: (*a*) that oscillations in the economic system must be regarded and treated as oscillations about a trend of economic growth, in contrast to earlier treatments by reference to a notional static equilibrium; (*b*) that the same set of influences in the economic system can and should be used to explain the working of both the trend and the oscillations; (*c*) that these influences are best interpreted in terms of the Keynesian variables, which is to say, the variables in the national income model; and (*d*) that the principal functional connections between the variables which are relevant to economic fluctuations (the behaviouristic or technological functions which I have earlier referred to) are to be found in " the double inter-relation between investment and income . . . the dependence of income on investment through the multiplier, and the dependence of investment on income through the associated changes in profit expectations."[1] (I have taken care to use Mr. Kaldor's words to describe that final point because, whilst one is tempted to counterbalance the single word " multiplier " with the single word " accelerator ", one has to remember that Mr. Kaldor, who is one of the principal theorists here involved, has accepted the " acceleration principle " as the appropriate form of the income-investment relationship only for the purposes of long-run analysis of economic growth; he still resists its application to the case of the short-run fluctuations of the trade cycle.[2])

Now, there is room for maybe endless differences, large or small, in the ways in which the four propositions which I have listed are translated into models of the economic system. The " acceleration principle " which Mr. Kaldor objects to for short-run purposes, for example, appears to be fundamental to Sir Roy Harrod's " basic dynamic antinomy ".[3] Within formally agreed areas there still remain problems as to the most appropriate relationship to be implied:

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[1] N. Kaldor, " Mr. Hicks on the Trade Cycle ", *Economic Journal*, December 1951, p. 833.

[2] N. Kaldor, " A Model of Economic Growth ", *Economic Journal*, December 1957, p. 601n; but see also *Economic Journal*, March, 1954, p. 54n.

[3] R. F. Harrod, " Notes on Trade Cycle Theory ", *Economic Journal*, June 1951, p. 262.

whether, for instance, in the case of the consumption function, a *de*crement of income has the same significance as an *in*crement of income about the same level, and whether the same quantitative relationship can span different ranges of time. And the fact that macroeconomics, linking with national accounting, deploys statistical rather than primary economic terms makes for uncertainty as to its exact analytical content.[1] If these terms are difficult to define in the clean and air-conditioned spaciousness of economic theory, how much more so in the dusty atmosphere of real life.

Nevertheless, from the point of view of economic prognostication, I believe that a very real advance has been made and that such matters as differences between theories and uncertainties of definitions are less worrying for this purpose than they may be for the theorists themselves. It is the fact that the theories all share a family likeness which is the most important feature.

The second feature in the mainly post-war development to which I would draw attention is the coming together of the economic theorists and the econometricians. Both parties have contributed to this *rapprochement* and for the simple reason that each has found the other useful.

The initial move must, I think, be ascribed to the econometricians, for it is they who have submitted their work to the disciplined influence of Keynesian or near-Keynesian macroeconomics where previously—if one harks back to the herculean labours of Wesley Mitchell and Professor Burns—no theory was allowed. The Econometric Society was founded in 1930 " for the advancement of economic theory in relation to statistics and mathematics "; and when present-day theorists refer to " the lag theory of the econometricians " the key-word is " theory "; the word " lag " is adjectival. The economist is in the saddle, though it may be ungrateful to describe the statistician by no kindlier term than " the patient ass ".[2] At the same time, we should take note of the inclusion by many of our trade cycle theorists—and notably Professor Hicks in this country[3]—of " lags " between connected variables as important elements in their model constructions. There is an obvious parallel between the process analysis developed by the theorists, wherein the functional *determination* of one variable by another is a dated relationship, and the analysis of time-series by the econometricians, wherein a variable at one date shows a statistical *correlation* with another variable at a different date. When the econometrician applies a model construction to his time series data involving a causal analysis, and when, moreover, the theorists' models are seen to share many of the same characteristics as those of the econometrician, the two procedures tend to flow together.

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[1] Cf. J. R. Hicks, *Value and Capital*, 1939, ch. 14.

[2] J. M. Keynes, in *Economic Journal*, September 1939, p. 559, and March 1940, p. 154.

[3] J. R. Hicks, *A Contribution to the Theory of the Trade Cycle*, 1950.

In the opinion of some economic theorists, of course, this development is a doubtful blessing. Sir Roy Harrod is so concerned to avoid any dilution of his " antinomy " as the prime cause of economic oscillation that he is prepared to leave his model deliberately incomplete rather than incorporate the lag elements which would be necessary to effect its completion.[1] On the other hand, Professor Hicks and the other theorists who have embraced the use of lag elements are obviously grateful for the help of the econometricians. For while their models themselves do not incorporate particular values for these lags, such information as the econometricians can provide is immensely valuable in indicating whether a model constructed in a particular form is likely to produce the oscillations required for it to appear realistic.

It would be grossly misleading of me, however, to cast my reference to this second post-war feature, the *rapprochement* between economics and econometrics, solely in terms of its value to the theoreticians in their construction of total models. The main work of econometricians, after all, is not to posit lags but to measure them, and to measure many other things besides. In Professor Tinbergen's words:

> The . . . step in any econometric analysis—ordinarily the one given most of the efforts of econometrists—consists in the *determination of the numerical values* of the coefficients in the relations between the variables.[2]

Now Professor Tinbergen is famous for his efforts at constructing complete econometric models of whole economies, a type of undertaking which seemed likely, in the early years after the war, to become a good deal more popular than has in fact been the case. Nowadays, Dr. Lawrence Klein is the chief exponent and it was an achievement, I think, to get Dr. Klein—following the second of his " econometric models of the United States ", published in company with Dr. Goldberger in 1955[3]—to come to Oxford to work on a similar econometric model of the United Kingdom. He and his colleagues at the Institute of Statistics have just begun to publish their results. As the title of their first article indicates,[4] this Oxford model is intended as a basis, or perhaps more appropriately, a machine, for forecasting. It is a formidable machine to look at, even without trying to understand the complex applications of the method of " limited information maximum likelihood " which went into its making. I shall need to ask, before the end of this lecture, what its place in the forecasting industry is, and whether it is likely that those of us who are still at the handloom stage will be thrown out of business.

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[1] R. F. Harrod, *loc. cit.*, p. 263.

[2] J. Tinbergen, " The Significance of Keynes' Theories from the Econometric Point of View ", in *The New Economics* (ed. Harris), 1947, p. 226.

[3] L. R. Klein and A. S. Goldberger, *An Econometric Model of the United States, 1929–1952*, 1955.

[4] See R. J. Ball, A. Hazelwood and L. R. Klein, " Econometric Forecasts for 1959 ", *Bulletin of the Oxford University Institute of Statistics*, vol. 21, 1959.

But I must postpone that question for the moment in order to comment upon one further aspect of the economics-econometrics *rapprochement :* the very substantial growth since the war in the study of particular sectors and limited aspects of the macro-economy. I have long held the view that one of the disadvantages of the tradition of " political economy " in which our subject grew up, in this country in particular, is that we have been disposed to giving too much of our time to the " popular issues " of the day and too little to the hard-grinding enquiries into the way in which our economy and its numerous component parts really work. (Needless to say, as a " professor of political economy ", I absolve myself from any of these strictures !)

But we have now reached the point where political economy itself demands unglamorous, dedicated service to the detailed facts of economic life. Macroeconomic analysis is primarily an empirical invention. While, in principle, it is possible for a theory of macroeconomics to be built up by rigorous aggregation of the analyses of microeconomic behaviour, such is not, in fact, the basis of the present achievement or the most promising source of further development. Partly, macroeconomics is a product of a kind of intuitive aggregation, as in the cases of the multiplier or the disputed accelerator. Increasingly, I would suggest, it is likely to be derived from hypotheses secured by direct study of the aggregates themselves, as in the cases of the lag elements in contemporary trade cycle theory to which I have already referred and of the " constancy propositions " relating to the capital/output ratio, the share of profit and the rate of profit which the researches, particularly of Professor Phelps Brown and his associates, have lately presented to us. These are not the products of deductive theory. Nor are they merely statistical checks on the validity of theory —though, as Mr. Kaldor has lately hinted with his new theory of economic growth,[1] they can be turned to advantage in that way. Their main prospective value, or so it seems to me, is in the direct contribution they can make to the construction of macroeconomic analysis itself, by giving it greater specification and thereby increasing its practical efficiency.

The greatly expanded study of limited aspects and particular sectors of the macro-economy is, then, the third sub-division of the *rapprochement* between economics and econometrics which promises well for the future of economic prognostication. It would take too long in a lecture of this kind to give a critical survey of all that has so far been accomplished or is in train, and a mere catalogue would be uninstructive. Professor Stone's brief review of the international record up to 1953[2] included from this country only his own work on consumers' behaviour and the several studies which had by then been made of prices and costs in international trade, of which Sir Donald MacDougall's is probably the best known. A review dated 1959 would have to include not only substantial output-flows from the National Institute, Cambridge, Oxford and this School, but contributions also from a wide geographical range, from Glasgow, through Manchester, Leeds and Nottingham, and on to Swansea. Nor should one take note of the academic institutions only. The enlightened attitude of our official statisticians at the Central Statistical Office and the Board of Trade should also be recorded: their efforts to make positive contributions to our stock of information instead of just throwing to us what drops easily out of the administrative sieves, and their readiness to put into regular flow-production the prototype developments from Cambridge and elsewhere.

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[1] N. Kaldor, " A Model of Economic Growth ", *Economic Journal*, December 1957, p. 593.

[2] R. Stone, in *Income and Wealth*, Series IV, 1955, pp. 41–51.

The third large feature of post-war development relevant to our subject of economic prognostication is the most recent and in some ways the most fascinating of all. I refer to the techniques, which you will associate particularly with the name of Professor Phillips,[1] for the analysis of the time-paths of income and other variables by the use of apparatus, theoretical and practical, borrowed from the engineers. It might, of course, be argued that I should have included this novel enterprise in the first category of developments which I enumerated, namely, developments in economic theory. For certainly this class of enquiry is theoretical and the analytical structures are of the same kind as those which I earlier dealt with: exploiting multipliers and accelerators and using particularly, of course, lagged responses between variables. There are, however, two reasons why I think this development deserves to be put in a class of its own.

The first and most obvious is the special techniques which are involved which in themselves constitute a new departure in economic method: closed loop systems, frequency response analysis and the use of electronic simulators. But, putting these spectacular features to one side, there is a second and very practical reason for putting this work in a separate class of its own. There is a difference of purpose. The theoretical developments which I first listed were primarily concerned with how the free, private economic system operates; this work is concerned first and foremost with stabilisation and control. It therefore has direct links with the second of the two features by which I earlier distinguished post-war from pre-war prognostication—our latter-day concern not with forecasting only, but also with planning, that is, with governmental policy.

It is this feature, I think, which makes the work of Professor Phillips and that of others on similar lines both distinctive and important. To forecast how the economy will move if the government does nothing is at all times a pertinent undertaking, but never more so than when it spurs the government into needed activity. Then come the questions not only of what kind of action and how much, but also at what dates. To one who has had some small experience of the time taken to get a policy across, the time taken to put it in train and the time taken for it to come to full fruition, a form of enquiry which starts by noting such differences between the potential and actual effects of a policy seems to have some realism. Moreover, it is one problem to keep an economy on the rails; it is a different and harder problem to get an economy back on to the rails when it has left them. An analytical apparatus which can distinguish between the accumulated past error, the current error and the rate of change in error, can certainly aid our thinking even if it does not pretend to solve all our difficulties.

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[1] See A. W. Phillips, "Stabilisation Policy and the Time-form of Lagged Responses", *Economic Journal*, June 1957, and the earlier publications by Phillips, A. Tustin and R. G. D. Allen therein referred to.

III

What has all this ten years and more of analytical industry contributed to the efficiency of present-day economic prognostication?

For the United States, where forecasting is a much more widespread activity than it is in this country, a just answer to this question would require a minor research study. I shall confine myself, perhaps unfairly, to two simple checks. The first is the performance of forecasters in relation to the United States recovery in 1958 as summed up by Professor Samuelson, himself one of the forecasters. In the *Financial Times* last October, he wrote:

> Few actually predicted it. Those few were primarily adherents of national-income model-building: when they added up the most probable changes in Government expenditure, in rates of decumulation of stocks, in businessmen's intentions to invest in plant and equipment, in family consumption spending, in public and private construction, and finally in our net foreign investment, the model builders generally concluded that sometime around the middle of the year the economy would stop receding.[1]

By contrast, Professor Samuelson noted that, if the model builders were the best predictors, the worst were those who relied most on *a priori* reasoning; and the third type of forecaster, " the statistician who studies past leads and lags ", also failed on this particular occasion, though the users of " statistical indicators " and " diffusion indexes " were usually more successful. Secondly, one may note that the latest achievement of the Office of Business Economics in the United States Department of Commerce, which they describe as " a major economic indicator ", is nothing more advanced (in economic terms) than a calculation of real national output by quarters[2]—an instrument whose only value, by itself, can be in enabling the prognosticator to nose forward when changes in trend have already reached significant dimensions. If these two instances are typical, they represent nothing very startling.

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[1] P. A. Samuelson, " The U.S. Economic Recovery of 1958 ", *Financial Times* 1st October, 1958, p. 8.

[2] *Survey of Current Business*, December 1958, pp. 10ff.

In the case of the United Kingdom, the instances are fewer as well as more familiar and I can deal with them a little more thoroughly. Almost invariably, the analytical structure is that of the national income model of accounting relationships and no more. In fact, I can find only two cases where the practice is consistently different. The first is the Klein econometric model at Oxford and at this date one can only say of it what is said of other high-energy piles: it has become active. The second case is Sir Roy Harrod.

Now there are many, economists and others, who would like to differentiate between the Harrod who is one of the most creative economic theorists of our time, and Harrod the controversialist whose articles on current economic affairs appear more or less regularly in *The Director* and the *Financial Times*. Certainly, Sir Roy has held strong and somewhat personal opinions on a number of post-war issues, and he has acquired an estimate of what is practicable among the conflicting purposes of our economic affairs (shared by many and opposed by many) which affects materially his assessment of what are the best things to do. All that, however, is beside the point as far as this lecture is concerned. The important fact is that the analytical content of what Sir Roy writes on current economic affairs bears a very close relation to his own construction of dynamic macroeconomic theory.

The test of the efficiency of the Harrod model in use, is, of course, by its results; no doubt some enterprising post-graduate student, at some date in the future, will present us with a thesis entitled, " The contribution of economic theory to economic prognostications: a case study in the writings of Sir Roy Harrod ". For the moment, however, a more readily available slant on Sir Roy's method of working is to be found in the article he published in the spring of 1956 on the 1954–5 boom.[1] The main argument of that article turns on two factors. The first is the working of the acceleration principle, which—with the multiplier—is one of the two key components in Sir Roy's theoretical model. The second is an institutional feature of our system to which Sir Roy then drew attention, namely, the conservatism (or, in his terms, the long-run view) on the part of the producers of investment goods in pricing their products: the primary effect of an inflationary gap near the full-employment ceiling was to cause, not a squeeze of consumption through rising prices, but rather a squeeze of investment through lengthening delivery dates for capital goods. The dynamic force of the accelerator, Sir Roy therefore argued, was to be found, not in the growth of realized investment, but in the excess of desired over realized investment, as illustrated by the lengthening of order books. Analysing the build-up of inflationary pressure in these terms, Sir Roy went on not only to argue the importance of choking off the excess of investment orders (as distinct from checking the tempo of realized investment), but also to question whether the current *rate* of investment could be maintained and whether the rate of increase in consumption demand would continue to be sufficient to absorb expanding output:

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[1] R. F. Harrod, " The British Boom, 1954–55 ", *Economic Journal*, March 1956.

> Important as it is to rescue the economy from the disadvantages of inflationary pressure, there is a still graver issue at stake. What of the succeeding slump ?[1]

That question, posed three years ago, does not seem irrelevant today.

Such conscious application of a dynamic analysis, whether explicit or implicit, is not, however, typical of the general run of economic forecasting in this country. The more normal form can, I think, be summed up in three characteristics. First, it is short-term: a review in the spring will not run beyond the calendar year and one in autumn will normally confine itself to the first six months of the calendar year following. Secondly, the framework for the assessment remains formally that of the national income model, using accounting relationships only. That, however, is not quite all. Thirdly, within the limited range allowed by the first two features, there *are* some functional relationships roughly deployed. And there is throughout, of course, a richness of information and quantitative content which were not, and could not have been, evident in comparable forecasts some ten or more years ago.

The newest of the available examples is, once again, probably the best to illustrate these points: the *National Institute Economic Review*. First, the chapter on " The outlook in Britain "[2] covers the year 1959 and does not go beyond it. Secondly, the form is the national accounting form. A quick check at the outset establishes that demand and production (in the absence of new Government measures) will rise only moderately in 1959, and there then follows a detailed exposition of this argument. After a section on wages, profits and prices—mainly to gauge their influence on consumers' purchasing power—discussion takes the standard order of consumers' expenditure, fixed investment, stocks and government expenditure. And so, exports having been dealt with earlier, to the general outlook. For my third feature—some rough deployment of functional relationships—one has to look more closely.

It is tempting to try and construct a " National Institute model ", but it would be unreasonable to do so on the material of a single issue. Certain of the relationships deployed on this particular occasion are patently temporary. The proposition, to quote only one example, that a rise in output " should principally be met by higher productivity, not by a corresponding rise in employment " is clearly proscribed, as is stated, by the phrase " from current levels ". Nevertheless, certain relationships are recorded which are evidently of more than passing moment and it is worth while pausing to note their character.

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[1] *Ibid.*, p. 12.

[2] *National Institute Economic Review*, January 1959, pp. 18–31.

There is, in the first place, a fairly straightforward construction for the determination of prices and money incomes, and hence of consumers' real incomes. The general course of total final prices is stated to be closely correlated, " after a normal lag ", with the trend of labour costs and import prices (though there is a subsequent reference to profit margins). For the early part of any one year, therefore, prices are already determined by the past. The import price component is further extended by the National Institute's own new index of current import prices, designed to give advance information on the United Kingdom import bill (and on the export incomes of primary producing countries), while labour costs are the combined result of wage-rates, the other factors affecting earnings (particularly hours of work) and productivity, of which wage-rates are regarded as autonomous, to be extrapolated from the rate of change in the immediate past (" The regular wage round ... has become an established practice ").

Secondly, there is some advance in the forecasting of consumption expenditure by separating durable and non-durable expenditure. Consumers' expenditure on non-durables is briefly related to consumers' real incomes. The analysis of the prospects for expenditure on durables is fuller and more complex, drawing partly on the Cambridge experiments with dynamic demand functions. The rough rule is quoted for the long run that purchases of durables increase about twice as rapidly as real incomes. There can, however, be marked cycles in such demand over short periods, affected particularly by hire-purchase conditions and purchase tax, the changes in the proportion of incomes spent on durables being partly matched by opposite variations in the rate of personal saving. In the end, therefore, there is less information of use to a one-year model than there might be for a model extending over a longer period.

The key problem remains, as always, that of the future of private investment. As the main factors governing stocks and work-in-progress, the National Institute gives the trends in business sales, which is an endogenous variable, and in business expectations, which is not. What of private fixed investment? All the known data on recent trends are recorded; industry's own predictions of investment spending (now available in different forms from both the Board of Trade and the Federation of British Industries) are drawn upon; reference is made to the inflow of building orders and to the National Institute's own enquiry into capacity-user; the point is recorded that " it is consistent with past cyclical experience that fluctuations should be greater in factory building, which results in a long-lasting product, than in acquisitions of plant and machinery ".[1] But all this is used, not to give quantity to a theory of investment movements, but as guidance on business expectations, which remain, as in the case of stocks, the governing and exogenous variable.

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[1] *Ibid.*, p. 25.

To sum up then: when earlier in this lecture I considered the post-war developments in macroeconomic enquiry which appeared to be potentially most helpful to economic prognostication, I listed three lines of advance: first, the establishment of a broad consensus regarding the constituents of a satisfactory macroeconomic theory; secondly, the coming together of economics and econometrics, resulting in a valuable feed-back from econometrics to economic theory, the construction of complete econometric models, and greatly expanded study of particular sectors and aspects of the macro-economy; and thirdly, the application of stabilisation and control theory to the analysis of the time-paths of interrelated economic variables. My brief survey of the actual state of current economic forecasting in this country shows that, with certain notable exceptions, the general form of forecasting appears to have been affected by only one part of these developments: that sub-section of the economics-econometrics *rapprochement* which is made up of partial and specialist studies. Is this the whole truth? And, if so, are we content to leave it at that?

IV

If the appearance as thus summed up is the whole truth, then we are excluding the use of econometric models, we are excluding any general theory of macroeconomic behaviour and we are excluding any possible theory of stabilisation and control.

Now as regards econometric models, I do not foresee them superseding present procedures for a very long time to come, though it will certainly be interesting and instructive to have the Oxford experiment running parallel. The development in economics which I have emphasized as having contributed most to the improvement in the quality of economic forecasting is the substantial enrichment of its content by the incorporation of new, specialized knowledge and information. Despite the complexity of the Oxford model—it incorporates 30 functional equations besides another seven identities and technical definitions—it is not capable of absorbing all that information. Nor, I think, can it be made to do so: too much valuable information has to be laid aside because it is not of a form which an econometric model can accommodate, even on the most procrustean terms. There is a second, allied obstacle. An econometric model is not fundamentally different from any other type of model. All alike consist of a system of precisely stated quantitative relations between variables, whose implications are then worked out by a strict, deductive logic. The econometric model, however, is an extreme example of a model which, because of its complexity, can only be worked out by mathematical processes—processes which have no clear economic counterpart.

In the present state of our macroeconomic knowledge, and given the limitations already noted on the capacity of econometric models to cope with information, this seems to me to be a very unsafe method of working. I recall a phrase of Professor Tinbergen's own: " In Holland we say that we don't want to reach the end point by night train, but by day train ".[1]

As regards the other two potential components—a theory of macroeconomic behaviour and a theory of stabilisation and control—my conclusion is different. In the first place, it is not quite true to say that they are at present altogether excluded. To make this point clearly it is necessary first to remind you how economic forecasting is done.

Sir Roy Harrod's frequent and lively excursions notwithstanding, economic forecasting is normally not a solo, but a group activity. The *National Institute Economic Review* represents one kind of group: a team of some eighteen workers in a single, full-time organisation, whose particular individual research activities are linked with their corporate forecasting. The *London and Cambridge Economic Bulletin* shows a group of a second kind: a bunch of independent university teachers who periodically assemble in order to pool their separate studies and experience of the economic situation and its parts. And the *Economic Survey*, though there is only a rather shadowy forecasting in its published version nowadays, represents a third form of grouping: a central professional nucleus in the Economic Section of the Treasury which draws on a wide range of contact and information in the other government departments. In each case, there is a fusing of individual research and experience into a group assessment.

Now, in conditions like these, though the national accounting model provides the common and agreed base—the readily accepted agenda, as I described it earlier—there is no reason why functional macroeconomic theories need be excluded. It is more than likely that some if not all of the contributing members of a group will be possessed of them, and, indeed, that they will impart some colouring to their views on the general economic situation. Why, then, do they not make themselves more apparent?

One possible reason is a failure to agree on their precise formulation. I reject this, not because agreement is likely to be easily come by (it obviously won't be), but because I do not think that agreement on a precise formulation is necessary. Indeed, provided that there is broad agreement on the structure of a macroeconomic theory—and such broad agreement I suggested earlier does exist—I should count it an advantage that there should be differences in the models which different individuals use. Most of the time, I should expect differing models, given that they are within the same family, to yield much the same results when applied to a particular situation. And where this proved not to be the case, it would be a gain to bring the differences out into the open.

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[1] J. Tinbergen, *loc. cit.*, p. 229.

The reason for the inhibition is to be found, I think, in a feature of contemporary economic forecasting to which I have already made reference, namely, its habitual restriction to what, from the point of view of macroeconomic theory, is an unduly short period: the twelve-month limit. For over such a relatively brief span of time the main components of macroeconomic theory lose a large part of their strength. The consumption-function becomes imprecise, while the accelerator (let us call it) becomes irrelevant. We do not need a theory of investment to forecast capital formation over the next six months; it is enough to look at the order-books.

At first sight, this restricted method of operating appears to have considerable advantages: it keeps forecasters out of trouble. And economic prognostication in this country has survived, on this basis, for some fifteen years. But I believe that we are coming to the end of the period when that is so, and that 1959 may well mark the date when economic prognostication is compelled to break new ground. (I say "compelled", implying a compulsion from economic events; it is arguable, witness the example of Sir Roy Harrod which I quoted earlier, that prognostication ought to have taken the further steps some three or four years ago.) Thus, it could be said, without any disrespect to a very fine performance, that the group of forecasters in the National Institute have been "first time lucky" in their *Economic Review*. They restricted their forecasting to the year 1959 and, for that year, they found that the expected decline in capital formation by private manufacturing industry is roughly offset by the planned increase in public investment.[1] The general stability of demand and income with which they conclude their analysis turns on the constancy of the investment aggregate. But are the rates of development in these two sources of capital formation the same? If not, what is the expected course of the fall in private investment, and what pattern of public investment, planned and practicable, is needed as an offset? What, in other words, is going to happen in 1960?

One can expect the National Institute to have something to say on this question in their second issue. Some comments from others are now appearing. Dr. A. R. Prest, posing as Chancellor of the Exchequer for the *District Bank Review*, accords 1960 a footnote when dealing with public works.[2] The *London and Cambridge Economic Bulletin* for March sees the threat of a fall in the output of the capital goods trades next year, and makes some brief but pertinent observations on the height of long-term interest rates and on the Government's misplaced emphasis on short-term capital projects in the public sector.[3] But is not this all a case of "too little and too late"? Now that the British economy has moved into an era of general freedom, which includes freedom to fluctuate, I suggest that prognostication will lose much of its value—it might even become misleading—unless it can raise its sights fully to the range of the fluctuation problem.

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[1] *Op. cit.*, p. 28.

[2] A. R. Prest, "If I were Chancellor", *District Bank Review*, March 1959, p. 8n. Mr. G. D. N. Worswick, in a second article with the same title, raises the more general problem of the rate of economic growth.

[3] *London and Cambridge Economic Bulletin*, March 1959, p. i.

The instruments available for mounting such an attack must be those which I have already listed: a more explicit application of a general macroeconomic theory to gauge the size and character of the problem which lies more than one year ahead, and a more conscious application of whatever insights the youthful stabilisation theory can provide, in getting the right measure and timing for the policies of control. It may be, of course, that it is impossible to capture and control " the volatility of entrepreneurial expectations "[1] within a usable analytical theory. But I do not think we should concede the battle without a fight. After all, if " explanation is simply prediction written backwards ", it is not only economic prognostication which is under fire. This is a testing time for economic theory also.

University of Bristol.

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[1] N. Kaldor, " Economic Growth and Cyclical Fluctuations ", *Economic Journal*, March 1954, p. 70.

Economic Growth and the Problem of Inflation[1]

By NICHOLAS KALDOR

PART I

It is a commonplace to say that the theoretical speculation of each generation of economists centres around the particular problems which are in the forefront of public interest. If problems of unemployment and economic stagnation were the main objects of speculation in the 1930's, it is only natural that interest should be focussed on economic growth and inflation in the 1950's. For the post-war age has become development-conscious and productivity-conscious to a degree that was unknown to earlier generations; also, the trend of rising prices has assumed an extent and a duration, in most of the advanced economies of the West, not previously encountered under peace-time conditions.

Yet while the centre of critical interest shifts with the general background, there is also a strong underlying continuity in the development of economic theory—the insight gained in the investigation of one set of problems in one particular decade is not lost sight of; it serves as the basis for the investigation of the new set of problems of the succeeding decade. There is, moreover, a certain natural rhythm in the development of knowledge: as the conclusions reached at an earlier period are gone over afresh, and reinterpreted in the light of new experience, they invariably bring to light fresh implications that were hidden at the time of their original discovery and which open new avenues for thought. In the course of this process the tools—the particular techniques of thought—are retained and sometimes improved upon, while some of the basic assumptions of the theories which originally employed them are discarded. A particular instance of this, of which I shall say more presently, is the application of the Keynesian techniques of analysis to full-employment situations. The existence of an under-employment equilibrium, where effective demand, and not the scarcity of available resources, governed the general level of production, was of the very essence of the Keynesian economic theory as originally put forward. Yet the key to some of the post-war theories of economic growth is the application of the same principle—the principle of effective demand—to full-employment situations, in which, according to their author (who, however, was himself not very consistent in this), these principles were not thought to have any application at all.[1]

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[1] This article contains the substance of the first of two public lectures delivered at the London School of Economics and Political Science on 6th and 13th February, 1959. The second lecture will be published in the next issue of this journal.

I believe I am right in saying that until recently it would have been incorrect to speak of a *theory* of economic growth. Apart from a few exceptions, of which the late Professor Schumpeter was an outstanding example, economists of the neo-classical school were predominantly concerned with problems of market equilibrium—with the pricing process, and not with the problems of growth; and the techniques developed by this theory were ill adapted for dealing with capital accumulation and growth as a *continuing* process. The most advanced part of the Walrasian general equilibrium theory—the theory of capital, as developed by Wicksell—was really concerned with the implications of a state of long-period stationary equilibrium; and in so far as this theory was intended to be applied to problems of growth and accumulation, it was through the device of comparative statics—by a comparison of *two* states of stationary equilibrium, one of which possessed a larger amount of capital than the other. Beyond a few vague generalisations such as that progress involved a continued increase in the stock of capital in relation to income and a falling rate of profit, these theories had precious little to say about a growing economy; and if these generalisations were themselves contradicted by experience, the contradiction was attributed to the influence of residual features which were outside the scope of the theory—to the " non-economic " variables, such as changes in " knowledge ".

With Keynesian " macro-economics " a way was cleared to a new approach to a theory of growth, but, apart from the rather isolated attempt of Harrod, most Keynesian economics for the first ten or fifteen years after the publication of the General Theory were more concerned with the explanation of cyclical fluctuations than with economic growth. These trade cycle models were sometimes made to revolve around a rising long-term trend, and thus earned the title " dynamic ", but these trends were tagged on to the cycle models and not themselves explained—nor was it ever properly considered whether the existence of a rising trend is compatible with the unhindered operation of the multiplier-accelerator mechanism on which the cycle models are based.

My own ideas in this field owe a great deal to the pioneering essay of Sir Roy Harrod more perhaps for the stimulus it provided to fresh thinking, than for its assumptions or conclusions; and this, I believe, holds good for several other people who have worked in this field, such as Mrs. Robinson and Professor Champernowne.

My first lecture is devoted to three major aspects in which this new approach to the problems of growth and accumulation radically departs from earlier theories, both Keynesian and pre-Keynesian. My second lecture will draw certain conclusions that follow from this analysis with regard to the causes and consequences of inflation.

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[1] Cf., e.g., *General Theory*, p. 112.

(1) The first of my three aspects concerns a generalisation of the Keynesian theory of income generation so as to cover both underemployment and full-employment situations.

(2) The second concerns the relation of technical progress and capital accumulation.

(3) The third, the relation of the rate of economic growth to the profitability of investment.

I am conscious that these three aspects do not suffice by any means to give a comprehensive account of the new growth theory, but they are necessary and I hope sufficient to support the propositions about inflation which I shall consider in the next lecture.

1. THE THEORY OF INCOME GENERATION AND FULL EMPLOYMENT

Keynes's theory of income generation—which can be summed up by saying that expenditure decisions determine incomes, and not the other way round, or, what comes to the same thing, that it is investment which determines savings, and not savings which determine investment—was the really novel feature of his *General Theory*, which revolutionized our thinking far more, in my view, than other features, such as his theory of interest. Yet I always had the uneasy feeling that, as originally put forward, it attempted to prove too much; if the general level of production is really determined by effective demand, irrespective of the available resources, the equilibrium level of employment could be anywhere—in *any* relationship that is, to the supply of labour. Yet in the history of advanced capitalist societies periods of severe unemployment were exceptional and not the rule; apart from periods of acute depression, unemployment did not appear to exceed a few per cent. on the average since the latter half of the 19th century.[1] (In the forty years, 1881–1920, in the U.K. it averaged less than five per cent., including both boom and depression years.) As the actual level of employment averaged 95 per cent. of the full employment level, this is unlikely to have been a mere coincidence; it strongly suggests that forces must have been in operation which operated on the relationship between effective demand and supply, or between the propensity to invest and to save, in such a way as to yield an equilibrium level of employment that was fairly close to, if not equal to, the full-employment level.

To some people this would provide a justification for preferring the classical theory to the Keynesian, and for arguing that the money and capital markets did, after all, behave in such a way as to adjust the level of investment to savings and thereby generate sufficient investment to absorb the savings forthcoming at full-employment levels of income.

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[1] This was not so, of course, in "under-developed" countries whose unemployment, however, was of a different kind—not a reflection of an insufficiency of effective demand.

I myself would regard this explanation as inherently implausible, and I might adduce, if called upon to do so, various bits of evidence for the view that it was the Keynesian and not the classical mechanism of income generation which was operative in the advanced centres of capitalism for periods extending long before the first World War. But I think there is a perfectly good " Keynesian " explanation for the prevalence of full-employment or near-full-employment conditions—and it is to be found in the fact that in a competitive society (I ought to say a *fairly* competitive society—I do *not* mean perfect competition) variations in the strength of demand cause variations in the level of prices in relation to costs; these variations in turn have a powerful influence on the community's propensity to save or to consume, and thereby adjust the level of effective demand so as to make it coincide with the available supply, as determined by the resources available. Keynes, in postulating that the *propensity to save* is given independently of the level of investment, and thus of the strength of effective demand, has implicitly assumed something which I am sure he would have denied if put explicitly—that is, that the level of prices in relation to costs, or in other words, the relation of profits to income, has no effect on the community's propensity to save. Yet it is a well known fact that the proportion of profits saved is very much greater than the proportion of savings out of wages, salaries or rentier income; the one is about ten times as large as the other. It is around 50 per cent. whereas savings out of contractual incomes are more like 5 per cent. (The reason for this is not merely or mainly that residual incomes are so much more uncertain and fluctuating than contractual incomes: it is that in the case of business profits, the long-term prospects of the enterprise are closely linked with the accumulation of internal savings; even though in the short period, business capital expenditures are not directly influenced by variations in *current* savings.) Hence a relatively high level of investment, by causing effective demand to be high, also causes profits to be high: and these profits themselves provide the savings for the finance of this investment. When effective demand is relatively low, profits are relatively low, and thus the propensity to consume relatively high. Hence the Keynesian determinants, the propensities to invest and to save, and the theory of the multiplier, are more likely to operate so as to determine, not the level of employment, but the distribution of income between profits and wages at full employment. The very fact that prices rise or fall under the influence of demand creates an automatic tendency towards full employment, at any rate within certain boundaries.[1]

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[1] This proposition bears only a purely superficial resemblance to the celebrated " Pigou effect " of which Professor Patinkin and others have recently written, and which relies on an adjustment in the propensity to save *via* the reduction of both money costs and money prices and the consequent rise in the real value of money balances. I do not believe in the " Pigou effect ", which is irrelevant to a world where money wages are not highly flexible in a downward direction; but it is curious that the many economists who have written about the " Pigou effect " have

To illustrate the nature of this equilibrium, and the conditions under which the forces analysed by Keynes lead to full-employment, rather than unemployment, equilibrium, I should like to make use of the time-honoured device of the "representative firm". This is assumed to behave like a small-scale replica of the economy as a whole —that is to say, it is assumed that variations in the output of this firm reflect equivalent variations in the aggregate volume of production.

I shall begin by drawing the short-period cost curves of this firm—average and marginal prime cost—in a perfectly orthodox manner as found in the text books (Fig. 1). But we must remember that our *prime costs consist of wage-costs only* (since raw material costs cancel out for the economy as a whole), and I assume money wages to be given.

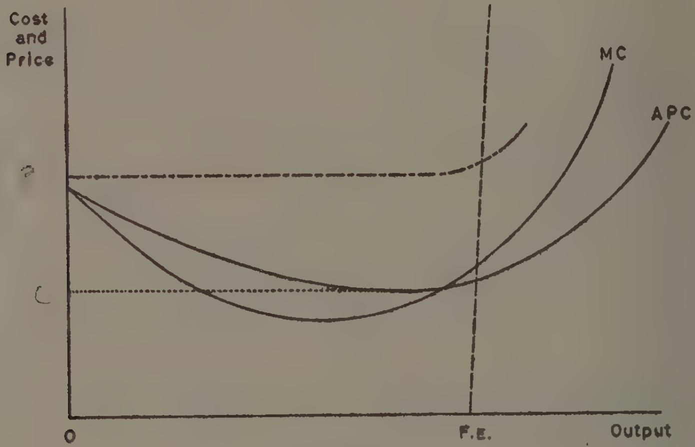

FIGURE 1

I should like, however, to introduce three amendments to this picture. The first is that we can ignore the falling portion of the *APC* curve and assume that average prime and marginal costs are constant, as shown in the dotted line in Fig. 1, up to a certain scale of production where they begin to rise on account of the more intensive use of capacity, or the need to resort to inferior capacity.

The second is that employment rather than physical capacity is the effective bottleneck setting a limit to production for the economy as a whole—i.e., it is assumed that the available capacity is more than sufficient to employ the available labour force. Hence the limit of

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not hit upon the fact that a fall in prices *relatively to costs* is bound to reduce the propensity to save far more powerfully than one which reduces both prices and costs in the same proportion.

production to our firm, which employs a constant fraction of the total labour force, is full employment (as shown by the dashed line in Fig. 1), which may itself be stretched somewhat through the use of over-time and the drawing-in of people like married women on the fringe of the labour force.

The third is that whatever the state of demand, our firm, for fear of spoiling the market, will not reduce prices, relative to costs, below a certain point. There is thus a *minimum margin of profit* which we can call the degree of monopoly or market imperfection, or whatever we like, remembering that it does not necessarily *set* the price, it merely sets a rock-bottom to prices. (In Fig. 1, the dot-and-dash line indicates the minimum price at the given level of prime cost per unit of output; the excess of this over the dotted line indicates the minimum margin of profit.)

With these amendments, the short-period supply curve to the firm, *S–S*, will be determined by the " degree of monopoly " line and the full-employment line, as shown by Fig. 2.

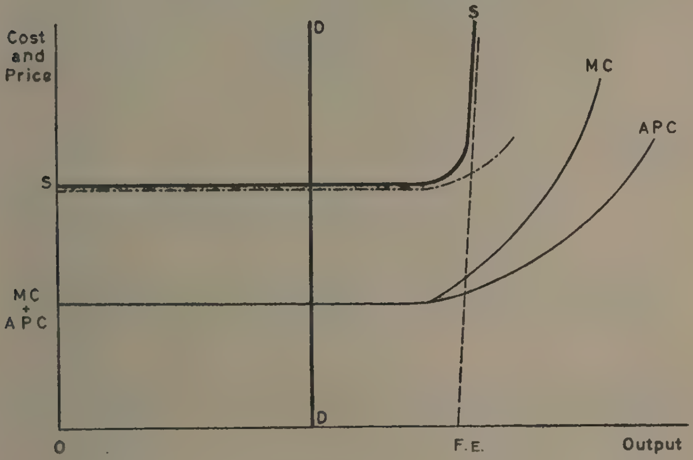

FIGURE 2

Keynes's " effective demand " curve (*D–D*), should be represented by a vertical straight line, when investment is regarded as a constant independent of output, and the proportion of income saved treated as a constant invariant with regard to the relationship of prices and costs. The equation of this curve according to the well known multiplier formula is

$$D = \frac{1}{s} I \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

where $s = \frac{S}{Y}$, the (linear) fraction of income saved, and I represents the level of investment. The intersection of the $D - D$ curve with the $S - S$ curve indicates the equilibrium level of output or employment.

If we assume, however, that s is not a constant but a variable, depending on the level of prices in relation to costs, we are replacing the above equation (1) with the following equation:

$$D = \frac{1}{(\alpha - \beta)\frac{P}{Y} + \beta} I \qquad (2)$$

where $\frac{P}{Y}$ represents the margin of profit as a proportion of the selling price (or the share of profits in income) and α the proportion of profit saved and β the proportion of other income saved.

Equation (2) yields a *forward-falling* demand curve (in place of a vertical curve) much like the Marshallian demand curve, since the lower the price in relation to prime costs, the greater the effective demand generated by the given level of investment. As shown in Fig. 3, this can yield either a full-employment or an under-employment equilibrium, depending on whether the demand curve cuts the supply curve in the horizontal section of the latter (as shown by $D - D$, with the point of intersection P) or in the vertical section (as shown by $D'- D'$, with the point of P').

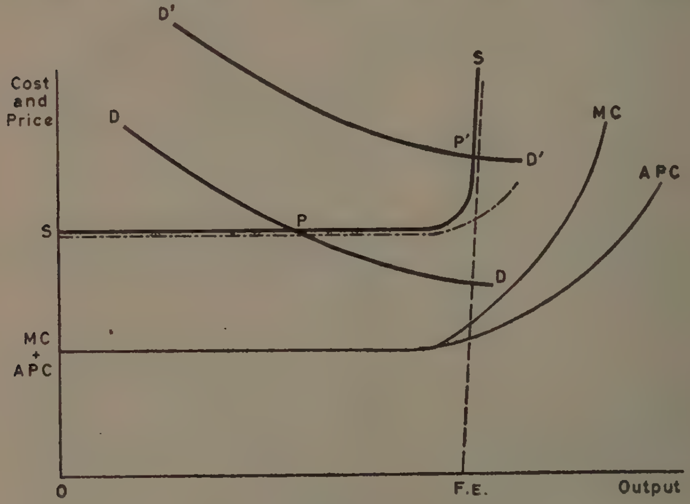

FIGURE 3

However, we must now also allow for the fact that not *all* investment is independent of output. Some of it is "autonomous" but the rest is " induced ", the latter only coming into operation when the degree of utilization of capacity permits a normal rate of profit being derived; in other words, when receipts cover average *total* costs (including " normal " profits). Allowing for this, our equation (2) becomes

$$D = \frac{1}{(\alpha - \beta)\frac{P}{Y} + \beta}(A + I') \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

where A denotes " autonomous " investment (i.e., investment that is independent of the current level of activity) and I' " induced " investment which will vary with the level of output and the degree of utilization of capacity.

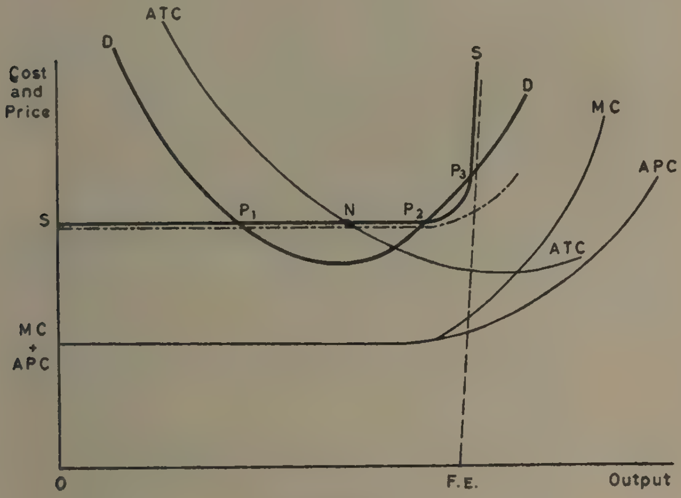

FIGURE 4

In Fig. 4, the curve *ATC* indicates average total costs (including " normal " profits) and the point N (where the curve *ATC* intersects the $S - S$ curve) the level of production which yields a " normal " profit on the existing capital equipment. Beyond N, any further increase in production will " induce " investment in the shape of additions to productive capacity, and it is reasonable to suppose that the increase in investment associated with an increase in output will exceed the increase in savings, for any *given* distribution of income. Hence equation (3) will yield a U-shaped demand curve; the curve will be falling up to N (when induced investment is zero) and will slope upwards to the right of N (when induced investment is positive). As shown in Fig. 4, this will yield multiple positions of equilibrium, P_1, P_2 and P_3, of which only P_1 and P_3 are stable positions whereas P_2 is unstable (since at P_2 where the demand curve cuts the supply curve from below, a small displacement in either direction will set up cumulative forces away from P_2 until either P_1 or P_3 is reached).

It follows that an under-employment equilibrium is only *stable* under slump conditions when induced investment is zero.[1]

It also follows that it is impossible to conceive of a *moving* equilibrium of growth to be an under-employment equilibrium. Such an equilibrium is necessarily one where productive capacity is growing, and where, therefore, induced investment is positive, and hence the $D-D$ curve slopes upwards and not downwards. It therefore postulates the equilibrium of the P_3-type and not of the P_1-type. In that situation, the profit margin must be above the minimum level, and the distribution of income will tend to be such as to generate the same proportion of income saved as the proportion of investment in output.

It only remains to add that all this is perfectly consistent with Marshallian orthodoxy—looking at it as a *short-period* theory of distribution. For all it says is that the level of quasi-rents in the economy will depend on the strength of demand, which is precisely what Marshall said. It is when we come to long-period analysis that we part company from the orthodox theory.

2. TECHNICAL PROGRESS AND CAPITAL ACCUMULATION

This brings me to the next point—the relationship of technical progress and the growth of capital, and the role of these factors in determining both the rate of growth of productivity and the propensity to invest.

Since the very beginnings of human society, man's ability to exploit the powers of nature to his own advantage meant on the one hand a continued increase in the density of population in a given natural environment and, on the other hand, a continued increase in the amount of material aids (of man-made instruments) used per unit of labour. This latter assertion is more difficult to substantiate than the former since there is no reliable measure of the *quantity* of instruments per worker, when the character of these instruments is itself continually altering. However, I shall beg all questions connected with the measurement of the quantity of capital—which has given rise to so many headaches, both in theory and as a matter of statistical measurement—and assert that whether we measure capital in terms of values deflated by some price index, or in terms of weight of steel and other materials embodied, or in terms of animal or mechanical power, or in terms of any other reasonable index that we may choose, we should get substantially the same answer—that as productivity rose, the amount of capital per head rose with it, and if we compared different societies at different stages of development, we should find a very close correlation between the value of output per head and the value of capital per head. As far as statistics can tell, there is very little difference in the capital/output ratio between rich and poor countries—they are not significant in relation to the very *large* differences either in terms of output per head or in terms of capital per head.

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[1] It is possible to assume of course, that the shape and position of the demand curve at P_1 is not determined by " autonomous investment " but by a negative constant in the savings function—i.e., that savings become zero at some positive level of income.

The main point is that technical change and capital accumulation go hand in hand, and it is not really possible to isolate the effect of increased knowledge or ability or know-how from the effect of the accumulation of capital. The rate at which a society accumulates capital depends on the rate at which it adopts improvements in productive techniques; in the absence of such improvements, and abstracting from the growth of population, the process of accumulation would be rapidly brought to an end. Nor is it possible to distinguish, in general, the technical change which is the result of some brand new discovery from that which merely represents an improvement in know-how—in the ability to use and to exploit knowledge which has already existed, in some shape or another, in the minds of some people. Further, just as technical progress causes accumulation, the process of accumulation stimulates the growth of knowledge and of know-how.

Hence it is useless to analyse the effects of capital accumulation in terms of a production function which assumes a given *state* of knowledge, and then assume that this function continually shifts upwards with the progress of knowledge (see Fig. 5).[1] Since the accumulation of capital necessarily takes time, and both accumulation and the progress of knowledge are continuing processes *through* time, all

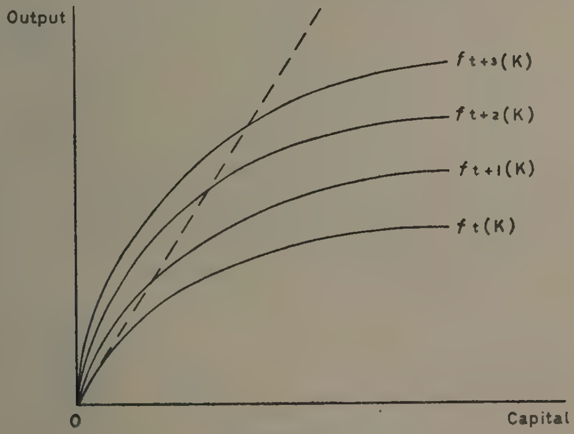

FIGURE 5

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[1] In Fig. 5, the amount of capital (per head) is measured horizontally, output vertically, and the curves $f_t(K), f_{t+1}(K)$, etc. denote the production functions at dates t, $t+1$, etc.

that we can say is that there will be some historical relationship between output per head and capital per head which embodies the effect of both technical progress and accumulation (such as, e.g., that indicated by the dashed line in Fig. 5). This relationship will not generally conform to the usual assumptions about diminishing productivity; doubling capital per man may well involve doubling or more than doubling output per man, thus giving a marginal productivity to capital that need be no smaller than its average productivity, or may even be greater. In other words, to obtain the linear-and-homogeneous production function required by the theory we must include " technical knowledge " as one of the variables, and treat it as a factor of production in an analogous manner to labour and capital (or land). But this is illegitimate, since technical knowledge is not a quantifiable thing; it cannot be bought or sold, and cannot have its marginal contribution imputed to it, any more than is the case with other unappropriated scarce agents of production.[1] This means, however, that in terms of labour and capital alone, the " production function " will not be homogeneous and linear, and the " marginal productivities " of capital and labour cannot serve as the basis of the determination of factor prices (or even be isolated !) since their sum will more than exhaust the total product.

Whether, with the accumulation of capital, the rise in output-per-head will be more or less than proportionate to the rise in capital-per-head will depend, not on technical factors alone, but on the *speed* with which capital is accumulated relative to the capacity to innovate, and to absorb changes in productive techniques. Whatever the rate of progress of knowledge, any particular society has only a limited capacity to absorb technical change in a given period. Hence it is more sensible to take as given, not the relationship between capital stock and output—the idea underlying the notion of a production function—but the relationship between the rate of accumulation and the rate of change of output. This latter relationship expresses the " technical dynamism " of the economy, since the more " dynamic " are the people in control of production (the entrepreneurs), the readier they are to change techniques and to introduce innovations, the faster will productivity increase and the faster will capital be accumulated.

It is the shape and position of this " technical progress function " which will exhibit features of diminishing returns. If we plot percentage growth rates of output per head $\left(\frac{\dot{Y}}{X}\right)$ along Oy and percentage growth rates of capital per head $\left(\frac{\dot{K}}{K}\right)$ along Ox, the curve will cut the y axis positively (since a certain rate of improvement would take place even if capital per head remained unchanged) but it will be convex upwards, and reach a maximum at a certain point—there is always a maximum beyond which a further increase in the rate of accumulation will not enhance further the *rate* of growth of output (Fig. 6). This means that the increase in capital (per head) will yield increasing or diminishing returns in terms of output according as the rate of accumulation is relatively small or large. If the rate of accumulation is less than *OP*, output will increase faster than capital, and *vice versa.*

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[1] As is well known, any function whatsoever in n variables may be regarded as a subset of a larger function in more than n variables which is homogeneous of the first order. But as Samuelson points out, it is not legitimate to treat as variables of the " production function " anything other than such inputs as are quantitatively measurable economic goods or services. (*Foundations of Economic Analysis*, p.84).

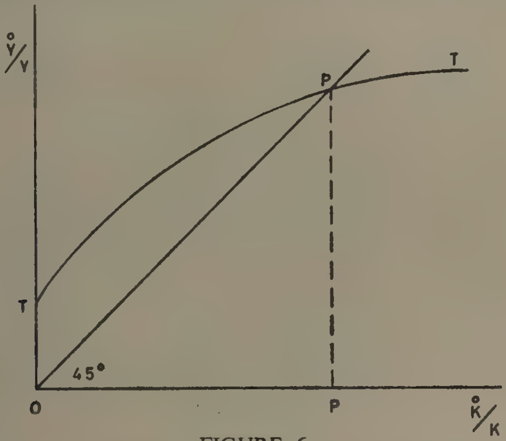

FIGURE 6

Given its " technical dynamism ", a capitalist economy, after a certain period of adaptation, will tend to settle down to a rate of economic growth and accumulation where the growth rate of capital is the same as the growth rate of output, since at this point the profitability of investment (the rate of profit on capital) will be neither rising nor falling. The historical constancy of the capital/output ratio, of the share of profit in income and the rate of profit on capital in advanced capitalist economies, is thus explicable in terms of forces which tend to bring these two growth rates (of capital and of output) into equality with one another.

On this analysis, it is the technical dynamism of an economy, as shown by the height or position of our technical progress curve, which is responsible both for making the rate of accumulation of capital and the rate of growth of production in the economy relatively small or relatively large. It explains why there is no long-run tendency to a *falling* rate of profit, or for a continued increase in capital in relation to output, either in slow-growing or in fast-growing economies. In economies whose technical dynamism is low, both the rate of accumulation and the growth of production will be relatively low, but in either case, growth can go on at a steady rate, without any necessary tendency to diminishing returns and thus to a gradual approach to a stationary state.

This view—that it is the technical dynamism of society which determines the rate of growth of both capital and output of the economy as a whole—is not meant to deny that any particular entrepreneur installing new capacity may not have a wide choice of techniques at his disposal, and that he will not make his choice on economic criteria in selecting that technique which promises to yield the highest rate of profit. But it does mean that the optimum technique will primarily depend on the cheapness and dearness of labour in terms of commodities in general and thus in terms of the various labour-saving instruments of production, and not on the prevailing rate of interest. In other words, the general level of the productivity of labour in the economy, which reflects the general state of accumulation *already* attained—since this determines the level of wages in terms of commodities—will ensure that in any particular industry the techniques adopted will be neither more labour-saving (and capital-using) nor less labour-saving than is appropriate to the particular stage of development of the economy as a whole. An entrepreneur, charged with, let us say, the construction of a hydro-electric dam, will employ very different techniques in India where labour is cheap than in America where it is dear. But there is no reason why there should be any difference in the prospective rate of profit earned on these techniques; if there is, the difference may be in either direction. There is no functional relationship between the price of labour in terms of commodities (which depends on output-per-head and capital-per-head) and the prevailing rate of profit on investments (which, as we shall see presently, depends on other factors).[1]

3. The Rate of Growth and the Profitability of Investment

Our analysis so far amounts to a denial that anything that may be termed the marginal productivity of capital or of labour has any role to play in determining the distribution of incomes or the ruling rate of profit. We have provided an alternative explanation for the determination of distributive shares, but we have said nothing so far as to the factors which determine the *rate* of return on capital. Is there nothing in the notion of a productivity-rate of interest, determined by *real* forces, and which governs the demand schedule for investable funds, as distinct from the monetary and psychological factors which govern the supply schedule?

The answer is that there is, and its existence in no way depends on the validity of the notion of marginal productivity, although it is not necessarily inconsistent with the latter notion. It is to be found in the dependence of the rate of profit on the rate of growth. I myself discovered this connection some 23 years ago,[1] when I was working on the theory of capital, on marginal productivity lines, and examined the case of an economy where all goods are capital goods—like a slave state, where labour is owned, and can be accumulated, in much the same way as material instruments. I found that the rate of interest in such a society is nothing else but the percentage excess of the goods produced over a period over the goods consumed in its production—in other words, it reflects the *net* productivity of the whole system. It is equal to society's maximum potential rate of growth: the rate of growth that would materalize if the whole of the net product were devoted to further accumulation.

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[1] The assertion that the existence of a functional relationship between the chosen technique (or rather the chosen amount of capital per head) and the price of labour relative to the prices of capital goods also carries with it a unique relationship between the chosen amount of capital-per-head and the ruling rate of profit is based of course on the postulate of a linear and homogeneous production function, and is not valid otherwise.

The same result was shown by von Neumann, in his famous model of general equilibrium.[2] Neumann's model does not necessarily require a " slave state ", but only the assumption (made by Ricardo and his school), that the supply of labour is indefinitely expansible at a constant real wage.

I think these results can be generalized and shown to apply to any steadily growing economy where the proportion of output devoted to investment, and the distributive shares are constant—where, in other words, wages and profits form a constant proportion of output. The second of these requirements is implied in the first when the propensities to save out of profits and wages are assumed to be given.

The derivation of this is quite simple. In a state of full-employment equilibrium, using the same denotation as before, it follows from equation (2)

$$\frac{P}{Y}=\frac{1}{\alpha-\beta}\frac{I}{Y}-\frac{\beta}{\alpha-\beta} \quad \text{......................(2a)}$$

In a steadily growing economy the ratio of investment to output depends on the rate of growth and the capital/output ratio. Hence denoting the rate of growth by G, and the capital/output ratio by K/Y:

$$\frac{I}{Y}=G\frac{K}{Y} \quad \text{..............................(4)}$$

But the rate of profit on capital, P/K, is by definition

$$\frac{P}{K}=\frac{P}{Y}\frac{Y}{K}$$

$$\therefore \quad \frac{P}{K}=\frac{G}{\alpha-\beta}-\frac{\beta\frac{Y}{K}}{\alpha-\beta} \quad \text{..........................(5)}$$

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[1] Cf. *Econometrica*, July 1937, pp. 228–230.

[2] *Review of Economic Studies*, vol. XIII (1), 1945–46, p. 1. (Originally read at a mathematical seminar of Princeton University in 1932 and published in German in 1938.)

When $\beta \rightarrow O$ (i.e., savings out of non-profit income are negligible or else balanced by non-business investment)

$$\frac{P}{K} = \frac{G}{\alpha} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5a)$$

In commonsense terms this means that the rate of profit depends on the rate of growth of the economy and the proportion of profits saved. In a steadily growing economy with constant distributive shares, the rate of growth of production will be the same as the rate of growth of profits. It can be shown moreover that equation (4) has a more general application if G is taken to mean the rate of growth of *profits* (whether or not the latter equals the rate of growth of output). For the rate of profit on investment can be defined as the marginal rate of profit on capital for the economy as a whole, i.e., the increase in profits ($\triangle P$) over the increase in capital ($\triangle K$). When business investment is entirely financed out of profits

$$\triangle K = \alpha P$$

Hence

$$\frac{\triangle P}{\triangle K} = \frac{\triangle P}{\alpha P} = \frac{\frac{\triangle P}{P}}{\alpha} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5b)$$

The important result which emerges from this is that the marginal efficiency of capital depends on how *fast* an economy is growing (or rather, on how fast profits are growing), and not on how richly or how poorly it is endowed with capital in relation to labour. This explains why the profit that can be earned by investing capital in advanced economies well endowed with capital is often greater than the profit that can be earned by investing in under-developed countries where capital is scarce in relation to labour.

I shall examine the implications of this on the problem of maintaining steady growth in the second lecture.

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Part II to appear in the following number.

Advertising Appropriation Policy

By Walter Taplin

I

About 95 per cent. of all advertising expenditure in the United Kingdom is aimed at promoting the sale of goods and services. It is in this purely commercial context that the question usually referred to in the advertising business as the problem of " appropriation policy " is examined here. This is simply the problem, presenting itself to any advertiser, of how much to spend.

The search for an answer would be simplified (though the problem would not be solved) if it were possible to measure with any accuracy the effect of advertising expenditure upon sales. But that, obviously, is very difficult. Many and various influences are at work upon the behaviour of purchasers in addition to the advertising directed towards them by the sponsors of a particular product or brand (not least of which additional influences is the competing advertising of other products or brands). It is also virtually impossible to assess quantitatively the sales effect of the so-called " creative " factor in advertising—the words and pictures. It is frequently possible, it is true, to observe that advertising has had *some* effect on sales in individual cases in the past and to forecast with confidence that it will have some effect in similar cases in the future. If this were not thought to be so the problem of appropriation policy could be regarded as solved in advance, and the right answer to the question how much to spend on sales-promoting advertising would be nil. But the positive problem of appropriation policy calls for a decision between more and less, and the limits of that decision may not be wide. A very small advertiser may consider doubling or trebling his appropriation, the total amount involved being a matter of a few hundred pounds. The largest advertisers, with an annual expenditure of several million pounds, and firms in those trades where advertising is a large proportion of total costs (perhaps thirty or forty per cent. in some cases), are more likely to want to know what would be the effect of an increase or decrease of ten per cent. or less. In fact thinking in terms of margins appears to be the common practice. As one advertising director put it, when questioned on the relation of advertising appropriation to required profit : " The first half-million we have to spend anyway. All the fights are about the last twenty thousand."

The possibilities of advance estimation in such cases are small and are frequently and more or less openly recognised by advertisers to be so. Nevertheless, decisions on appropriation policy must be made and in fact are made in business every day. Our purpose is to examine how these decisions are made, to note common factors in them, if any, and to list some of the more important issues left open for further study.

In the course of the inquiry on which the present article is based, more than forty persons having responsibility for decisions on appropriation policy were interviewed. Only advertisers were consulted, through directors or managers having a special interest in advertising and marketing questions. The opinions of agents and media owners were not sought in connection with the study. The firms consulted were in no sense a representative sample of advertisers. The firms selected for interview were those who were thought to be seriously interested in the question, to be willing to discuss it with an outside observer, and to have positive ideas about answers.

II

For the purposes of advertising appropriation policy, brands or products may be divided into three main categories, according to whether they are (*a*) new, (*b*) established, or (*c*) declining. In the case of a new brand, being introduced on the market for the first time, the appropriation for some months or years may include an element of investment. In some trades where advertising plays a particularly prominent part in the marketing process, for example, cosmetics and proprietary medicines, the investment in the brand name may be so heavy that the break-even point between sales and expenses may be several years from the original introduction. This investment element, which may also be present though not dominant in advertising expenditure over a much longer period, is in a few exceptional cases so effective that very little else is necessary and even " reminder " advertising is superfluous. Several instances were found in the course of the inquiry of very well known brands or groups of products of a single manufacturer (two at least were in the category of " household words "), in which sales were steady or increasing but present advertising expenditure was negligible.[1] On the other hand a big change in advertising strategy for an old brand may have something of the investment character of expenditures incurred in connection with the launching of a new brand, and in particular may wipe out profits for a year or two.

In the case (*b*) of established brands, there is a tendency for the process of making the annual decision on appropriation policy to settle into a routine. The inquiry included a critical study of these routines. Policy in (*c*), the advertising of declining brands, may vary between attempts to arrest the decline and attempts to accelerate it. Thus in the case of a firm wishing to retain the general goodwill associated with the brand name, the advertising aimed at slowing up its disappearance; and in the case of a firm which wished to transfer the plant devoted to the production of a declining brand to more profitable employment, though without giving undue offence to old customers, the advertising was sharply tapered off. Where multi-product firms are concerned there is a further tendency for differences in appropriation policy between brands to be ironed out in the total and the advertising appropriation as a percentage of turnover for the firm as a whole to settle down at a conventional or recognised level, so that the whole situation approximates to case (*b*).

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[1] The names of these or any other individual products cannot be given, since all the information provided by advertisers for the purposes of this inquiry was given in confidence and on the understanding that every effort would be made to ensure that such evidence should not be traceable to its origin—an understanding which was regarded as reasonable in the context of a highly competitive branch of business.

In addition to this three-fold classification of cases, there are a number of other general considerations affecting the question of appropriation policy.

The first such consideration is that marketing, properly regarded, is not an operation separate from and subsequent to manufacturing. Marketing considerations influence the manufacturing process at all stages. Marketing possibilities will be considered even before productive equipment is installed. But in many of the cases examined, particularly cases where marketing costs were small relative to manufacturing costs (though not necessarily small absolutely), selling activities, and advertising in particular, were regarded as a sort of subsequent boost given in the case of products whose material qualities did not appear adequate in themselves to stimulate sales, or did not stimulate them quickly enough. Such an attitude to marketing is liable to reduce the consideration of advertising appropriation policy to an emergency operation. This is not to say that the manufacturer who places emphasis on the quick-acting, shock-producing and flexible nature of advertising is always and necessarily wrong. It is possible to argue that these are its main commercial virtues.

A second general consideration is the relationship between the importance attached to the loading of plant and the amount to be spent on advertising. This is in effect only a particular case of the general proposition that marketing considerations should be taken into account at all stages of production. In instances where plant calls for intensive use if it is to be most economically employed, marketing, even though it may be continuously considered, is nevertheless necessarily treated as the servant of the production departments of the firm. But in the cases examined the question of the right balance between manufacturing and marketing departments was posed in a great variety of ways and admitted of an equally great variety of solutions, including the rather surprising case where marketing considerations were so heavily dominant that the whole manufacturing process was put out to contract and the firm's internal organisation concentrated on promotional matters.

Equally important in setting up the general framework of the appropriation policy question, and thus taking the first steps to its solution, is the need to relate advertising expenditure to all other marketing factors—such as quality, design, packaging, price, choice of outlets, direct selling effort and range of products. This is the question of the " marketing mix ", the right combination of a number of variables. It is a problem which is obviously very difficult to solve and to keep solved in a changing and competitive market.

The matter is further complicated by a technical problem, which absorbs a great deal of time and effort in actual practice. This is the problem of the particular ways in which it is proposed to spend the appropriation, the question of the " media budget ". The proportions in which the expenditure on advertising a particular brand is divided between newspapers, magazines, television, posters and so on is known within the business to be of great practical importance. The appropriation may be built up from a detailed schedule of media expenditures instead of being envisaged as a total decided first and subsequently carved up between them. Or the process may be started from both ends at once, financial policy and technical detail, and meet in the middle.

It was not the invariable practice, even among major advertisers, to consider jointly all the marketing factors listed above. Even in cases where most of them were put under the surveillance of one official, a marketing director or marketing manager, there was still a frequent reluctance to consider pricing on the same footing as all other marketing factors, and appropriation policy discussions might be carried through on the assumption that the price was fixed, even though differing sales effects from varying combinations of advertising and price were a real and practical possibility. Again it need not be assumed that in thus separating price from the other variables in the marketing mix the manufacturer is necessarily being foolish. The production considerations limiting the price of the product and the calculable effect of price changes on demand may be of an entirely different order from the more mysterious effects of changes in the shape of the container, a widening of the choice of outlets, a new direct selling drive by commercial travellers, or an advertising campaign.

III

The first common factor to emerge was the practice, widespread in the case of established brands and even fairly common in the case of the introduction of new brands, of regarding some particular percentage of turnover as being a rough indication of the right level of advertising appropriation. The second common factor was the practice of treating advertising as a special type of cost, possible variations in which are separated from, and often considered *after*, all other items in the budget and only *before* profit. In a majority of cases firms said that they followed both of these practices. This may at first sight appear to involve a contradiction, since in formal terms the two practices seem to be mutually exclusive. But in fact there probably was no serious contradiction, since the two concepts were not applied simultaneously. The conventional ratio between advertising and sales was most often taken as a starting point for appropriation discussions and possibly modified, whereas the adjustment of advertising appropriation and expected profit usually came at the end. These two major matters will be dealt with in Sections IV and V.

Before that, it is convenient to clear out of the way a number of incidental questions, to which reasonably reliable answers were found in the course of the inquiry, and which throw some useful side-lights on to the main subject-matter.

(1) The origins of the process which ends with the fixing of an advertising appropriation were examined with some care, and something of a pattern emerged in the case of established products. A procedure usually initiated by the advertising manager and completed by the board of directors, with the date of decision falling about three months before the opening of the sales year, was usual. The need to book newspaper space, television time, and so on, in advance, seemed to be the main influence on the time-table, and, in the nature of the case, consideration of any impact of the behaviour of competitors on appropriation policy was quite sporadic and not confined to any one period of the year. The period covered by the appropriation was usually one year.

(2) The extent to which competition affected appropriation policy-making, considered as a planning operation, varied greatly along a scale having at one end market leaders who ignored the advertising of the other sellers and at the other end firms in highly competitive markets with heavy advertising where the appropriation was kept continuously under review so that it might be revised as frequently as booking schedules with the media owners would allow. It so happened that trades in which the second extreme of procedure was considered appropriate—those with a high advertising proportion of total costs, such as toilet goods and some kinds of foods and drinks—were often those in which the flow of production was readily variable in response to demand, either through the simplicity of the process itself or because capacity could be switched easily from one brand or product to another. The advantages of advertising as a marketing instrument—flexibility and suitability as a shock transmitter—are present in all types of production. But some kinds of production are themselves sufficiently flexible for advertising to be resorted to most readily and its special marketing advantages most fully exploited.

(3) The case of purely advertising considerations playing a positive part in alterations in the market strategy of the firm did not seem to be of any importance. The mythology of advertising, of course, contains suggestions that some sudden inspiration or heaven-sent slogan may alter the whole course of a business.[1] The nearest approach to this in practice appears to be made in the case of brands where a particular advertising device has been found *in practice* to be highly effective in stimulating and sustaining sales over a considerable period. Only one such case was found in the course of the inquiry, and it was clear that the process was no overnight revolution.

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[1] See for example, H. G. Wells, *Tono-Bungay*, 1909.

(4) The argument that, taxes being high and advertising expenditure being allowable as an expense before tax, careful calculation would scarcely be worth while, is familiar. It is difficult to believe that it is not influential in forming general attitudes, but there was no indication that it very explicitly affected the detailed study of appropriation policy in the case of the firms consulted. And, as a matter of history, post-war American experience shows that, while corporation taxes have varied widely from year to year, advertising expenditure has gone steadily upwards.

IV

The practices of (*a*) regarding advertising appropriations as a fairly stable percentage of sales and (*b*) treating advertising expenditure as a discretionary deduction from profits rather than as an unavoidable cost of marketing were so common, and so many minor practices were connected with these two main ones, that an attempt was made to trace the main reasons for their frequent occurrence.

Of the firms consulted, 35 in fact regarded a named percentage as " about right " and this fact in itself produced some resistance to departures from the percentage, even though the firm in general tried to keep a fairly flexible attitude to appropriation policy. Those who indicated that this resistance was quite deliberate were less numerous, some nine in all, and the instances in which the reasons for the conventional percentage could be traced out in detail were very few. There appears in any case to be a tendency for competition between similar products to lead to similar appropriation percentages for each of the competitors and this was occasionally quoted as a major factor.

In only one case was the reason for this fully and explicitly stated, but it was sufficiently significant to be recorded. The firm concerned operated in an oligopolistic situation, selling products which were heavily advertised. It professed to dislike competitive advertising, but saw no possibility of reducing the proportion of its own costs devoted to it without losing sales to its competitors. At the same time it had occasionally refrained from undertaking additional advertising commitments, even when it was reasonably sure that new campaigns would have an initial effectiveness in increasing sales. The reason given for this was that its sales and advertising expenditure figures were sufficiently well known to its competitors for any marked change in the relation between them to be readily detected, so that the new campaign, if effective, would be imitated by these competitors and the appropriation percentage tend to settle down at a higher level than before for each of the competing firms. Since the possibilities of an expansion of total demand for the products concerned were not thought to be great, it was easier to leave the appropriation percentage undisturbed and concentrate only on the possibilities of achieving a greater sales effort by variations in the " creative " approaches and media schedules available within that percentage.

The practice of having conventional percentages of sales which are regarded as proper indicators of future advertising expenditure appears *prima facie* to be an illogical practice, in effect treating advertising expenditure as an effect of sales whereas common sense suggests that it should be regarded as a potential cause of sales. But a number of respectable reasons were advanced in its favour.

The first occurred in cases where advertising formed a large proportion of total cost and where economies from larger scale working were not expected. Room for manœuvre was therefore strictly limited. Any large increase in the advertising appropriation in such cases (we have already seen that, owing to the difficulty of measuring sales effects, small variations are little more than shots in the dark), could only be made by radically altering the whole production and distribution process or possibly wiping out profit for a period of years with no certainty of compensatory gains later. And while it is true that these difficulties would be unlikely to be so acute in the case of a possible large decrease in appropriation, it could nevertheless safely be assumed that any firm in this general category, that is to say one with a heavy advertising expenditure in relation to turnover, would also be reluctant to reduce the appropriation percentage for fear of losing sales.

A second consideration which was quoted in some cases as limiting the scope for changes in advertising appropriations was the relative inflexibility of other costs. A particularly striking case in point was that of oil production. The whole operation of production and distribution is dominated by the necessity for the flow from the refinery to be as smooth as possible. Departures from the normal advertising appropriation percentage for the products of the refinery as a whole would have carried with them the possibility of a change in demand which might be reflected in the need for adjustments of production balance at the refinery at more frequent intervals than before. Changes in the demand for some of the oil products might possibly be accommodated within the total flow, so that variations within advertising appropriation from year to year could be ventured. But no such variations would be possible in the case of the top grades of motor spirit, on which the bulk of the advertising of the oil companies is concentrated. Consequently the appropriation percentages for those grades and, since they dominated the production process, for oil advertising as a whole, turned out to be very stable.

Again, in cases where raw materials accounted for a considerable proportion of total costs and were only available in markets where the activity of a single buyer could have little effect on prices, there was a reduced possibility of securing compensating economies to offset an increase in advertising appropriation. It is, of course, conceivable in such cases that the cost of an advertising campaign might simply be covered by an increase in selling price, the ratio of advertising to sales remaining substantially unchanged. But even in cases where demand elasticities might possibly have permitted such action, direct statements that such procedures have been employed were not elicited in the course of the inquiry, except in three instances. There was a further stage of difficulty in making upward adjustments to the advertising appropriation where a generally inflexible cost structure was combined with a high advertising percentage. Room for manoeuvre was yet further restricted when these two inflexibilities were combined with a situation in which no one seller could safely depart from the existing scale of advertising for similar products without running the risk either of losing his part of the market if he reduced the appropriation percentage or inviting an equal and self-stultifying response from his competitors if he increased it. In such cases, the other costs closed in, as it were, on the advertising percentage and held it more or less fixed.

To these reasons for a fairly firm appropriation percentage, arising on the supply side, may be added the possible inflexibility of demand. Conservative consumption habits are more likely to stabilise the demand for products than for brands, so that there is scope for the enterprising advertiser even in notoriously immobile markets, such as that for mass-produced canned foods; but a number of instances were found where loyalty to a particular brand had settled down apparently immovably so that variations of the advertising appropriation had been found through experience to have little effect on sales either way. In such cases the tendency to reduce advertising expenditure or eliminate it altogether was obviously stronger than the tendency to increase it, and in a few cases this tendency had been fully worked out, advertising becoming negligible while the demand for the brand remained stable. But inertia, and the fear of offering too great a temptation to competitors to try to break in, usually combined to keep some kind of advertising in existence and to fix the appropriation at a conventional percentage of sales, which in this case meant a familiar absolute figure.

The arguments set out in the preceding three paragraphs were all advanced by advertisers who were aware of a problem of appropriation policy, and were sufficiently interested in it to provide reasons, or at any rate rationalisations, for their own practice of keeping close to familiar percentages of sales. In every case experience and special circumstances were adduced in support of existing procedure. Nevertheless a detectable pattern emerged. Not only were conventional appropriation ratios normally observed, but more attention was paid to the maintenance of advertising expenditure at a *percentage* of turnover than to the absolute size of advertising expenditure. So marked was this relationship in the cases examined, that it is tempting to venture the generalisation that large firms spending large absolute sums on advertising follow mechanical procedures in settling the appropriation, and that most of the systematic thinking on the subject is done by relatively small firms, with quite modest absolute expenditures on advertising but with high proportions of advertising in their total turnover. Obviously the temptation to press the generalisation too far must be resisted, since examples can be found of very big businesses which have both high advertising appropriation ratios and high absolute advertising expenditures, and some of these have given plenty of public evidence of the fact that they attach great importance to appropriation policy. Moreover, a clear distinction must be made in the case of multi-product firms (32 out of the 42 examined in the course of this inquiry) between brand policy and appropriation policy for the firm as a whole. In every case brand policy was more flexible than general policy. But what is less widely known, and what this inquiry has tended to confirm, is that some exemplary procedures for determining advertising ratios to sales are made in precisely those areas of business, relatively small in scale but bulking very large in public controversy, which are not normally held up as models of anything—namely cosmetics and patent medicines. In these cases there is seldom anything conventional or mechanistic about the procedure. It begins with the sales task to be done and it proceeds to the settlement of the appropriation percentage appropriate to that task, whatever that percentage may be.

V

The second main tendency, that of treating the advertising appropriation and expected profit as a sort of joint residuum, so that one can be increased only at the expense of the other, has long been recognised in the working handbooks.[1] The inquiry indicated that it was extremely common. Of those firms whose practice was examined, 32 indicated that they regarded advertising appropriations in this light. It is highly probable that, in the case of the majority of firms, whose approach to the problems of appropriation policy is less deliberate than that of the ones selected for interview, the " joint residuum " procedure is still more common.

The reasons for the practice range all the way from sheer short-sightedness to a deliberate decision on how much to venture on an activity whose results are deemed to be a matter of pure uncertainty. It was not always easy to discover which of the firms covered by the inquiry habitually left advertising expenditure for consideration at the end of the budgeting process, and treated it as a joint residuum with profits. Only six stated openly that this was their practice. But in some twenty other cases there were indications that this was the light in which advertising expenditure was viewed, and in several others it appeared likely that those responsible for the final decision, as distinct from advertising managers or individual advertising directors, were not inclined to regard advertising as an ordinary cost.

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[1] Cf. Lane's *Advertising Administration* (*Principles and Practice*), ed. Sandbrook, 1951.

Only in eight cases—all of them with very high appropriation percentages—was the joint residuum argument rejected as invalid.

The few cases in which those questioned showed no awareness of a possible absurdity in such a procedure included both those whose belief in the sales effectiveness of advertising was nil and those who attributed to it almost magical powers. The seller who had no faith whatever in the power of advertising to promote sales was perfectly willing to cut it out after a bad year, so as to be able to budget for a healthier profit in the next year. In his case the argument that advertising on a sufficient scale might itself lead to higher profits was not acceptable. And at the opposite extreme the seller who held that anything could be sold provided it was heavily enough advertised, and provided the firm was willing to cut down profits, was not disposed to accept the argument that in certain circumstances, for example those of severe trade depression, historical evidence tends to show that an increase in advertising expenditure may be a waste of money. So far as concerns any serious attempt to arrive at a reasonable appropriation policy, these extreme cases need be mentioned only to be dismissed. But they are not uncommon.

A tendency was also discovered, particularly among smaller firms with small resources, to adopt a roughly circular procedure. An advertising campaign is tried, it is apparently successful, sales of the product increase, the joint residuum available for profit or further advertising expenditure increases, larger sums are spent on advertising, and so on. In such cases there is always a possibility that the expansion of sales in the later stages might be achieved *without* further advertising expenditure, and in one case this possibility was explicitly recognised. But the additional advertising was undertaken just the same, on the assumption that, since the money was there, the risk that sales might fall if advertising expenditure was not maintained or increased was not worth taking.

It is clear that there is an element of irrationality in the foregoing approaches to the "joint-residuum" practice; and it appears likely that they are instances of an attitude of mind that is frequently found within businesses which have funds available for further spending or investment. That is to say, such individuals or firms may be inclined to adopt different criteria for the disposal, by "ploughing back" and so on, of funds arising within the business, from those which they would recognise as applying if similar sums had to be borrowed in the market.[1]

All other instances of the "joint residuum" practice examined contained a larger element of deliberation. In one case the practice turned out to be completely reasonable and had indeed resulted from a highly intelligent approach to the problem. This was the case of a firm (with a very small advertising expenditure relative to its turnover) in which the publicity value of maintaining a healthy dividend was regarded as more important than the direct advertising of its products. In this case all aspects of publicity were clearly being reviewed in relation to each other as part of the system of financial control, and profits themselves were regarded as having an advertising aspect. The accountants thus had a very big say, but, far from applying mere rules of thumb, were making intelligent use of concepts which were both flexible and progressive.

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[1] I am indebted to Professor Sir Arnold Plant for pointing out this implication.

Since an element of speculation in almost all commercial advertising is difficult to avoid, and since most advertisers explicitly recognise it, it was not surprising that a number of cases emerged in which an attempt was made to isolate the speculative area. The first and most obvious step is to distinguish declining brands and established brands on the one hand from new promotions on the other, a speculative element being obviously present in the case of new promotions. One of the most interesting cases of all those examined included one of the most striking (and incidentally, successful) advertising campaigns of recent years. In this case the campaign was launched on a large scale, without pilot operations and with a rapid approach to the maximum expenditure, on the clear understanding that the whole appropriation might be wasted. It is sufficiently clear that in such a case there is no absurdity in regarding the budgeted advertising expenditure in the first instance as a potential drain on profits. In such cases there is practically nothing of that small element of " insurance " which can be conceived as being embedded in the appropriation for an established product which has been consistently advertised over a series of years.

A case intermediate between a highly speculative operation, like the foregoing, and that of an established brand with a long history of close correlation between advertising and sales, is that of the multi-product firm with a considerable experience of new introductions and a well established budgeting system. In such a case advertising may be regarded as a cost, and a certain amount of cost data may, in the case of a new introduction, be taken over from the records of similar brands already established. Even in this case the advertising expenditure must stand out clearly as an exceptionally speculative factor. Nevertheless the manufacturer need not regard it as a potential 100 per cent. waste, unless his previous experience with similar brands in the past (which presumably includes a percentage of successes) is to be deliberately ignored.

Clearly the smaller the possibility of estimating the effect of advertising on sales, that is to say, the nearer the case under consideration approaches to the condition of pure uncertainty, the less irrational will be the " joint-residuum " treatment of advertising and profits.

It would therefore appear that although the practice of regarding advertising appropriations as a deduction from potential profit is common, it is only marginally prompted by a liking for speculation.

When it is a rational procedure, as it appears to be in the great majority of cases, it arises from the simple facts (*a*) that advertising is thought to have some effect upon sales, and (*b*) that no precise measurement of that effect has been possible.

VI

The only two ways out of the impasse which suggested themselves to those who were questioned in the course of the inquiry were closely related to each other. They were (1) a much more purposeful and widespread attack on the problem of measuring the sales effectiveness of advertising, using the known techniques of market research, and (2) a deliberate decision to hand over the whole problem of appropriation to specialist technicians for a fairly long period, in the hope that entirely new methods might be devised and applied.

It must be emphasised, in connection with course (1), that many of the individual case studies which are available are not disinterested. They amount to *ex parte* attempts by advertisers and advertising agents to show that expenditures on advertising have in fact been directly linked with more than proportional increases in profits. Information of this kind is not hard to get. Firms quite often volunteer it. But it may still be usable.

The large number of special studies of this nature analysed by Professor Neil Borden[1] at least demonstrate the possibility that measurement might not be out of the question. Possibly still more promising is the approach outlined by Paul E. J. Gerhold to the American Association of Advertising Agencies in 1957.[2] This was simply a demonstration of the relation between changes in advertising expenditure and changes in sales for individual products in the various *territorial* divisions of the United States. The consistency of the " S "-shaped curve which emerged, with sales rising slowly in those areas with the smallest increases in appropriation, then sharply in the areas with higher increases, and then tailing off where increases in advertising expenditures per unit reached still higher points, was sufficiently marked to prompt further examination of this simple " territorial " analysis. And the fact that such an approach is particularly relevant in a large country like the United States, with very marked territorial differences, does not seem to have prevented its experimental adoption by a group of advertising agencies in Switzerland. From these beginnings it seems possible that a systematic approach to questions of appropriation policy might be built up. Whether it could be extended from the relationship between *size* of the appropriation and sales to the relationship between sales and the *composition* of the appropriation (the media budget and the " creative " devices used) is another matter. But at least the purposeful development of the simpler measurements would seem to lead in that direction.

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[1] In *The Economic Effects of Advertising*, Chicago, 1942.

[2] Paul Gerhold, " Measuring the Productivity of Advertising Dollars ", in *Papers from 1957 Regional Conventions*, published by the American Association of Advertising Agencies.

It is unlikely that the time and money spent on trying out course (2) would be entirely wasted. Marketing experts have already spent more time demonstrating that the problem of measuring sales effectiveness is insoluble than in actually carrying out large-scale experiments, which is all the more surprising in that small-scale experiments have often produced interesting results.

Further experiment along this line, which would involve giving independent specialist technicians a free hand to tackle the problem of measuring sales effectiveness in their own way, without further direction or interference from the general management of the firm, would amount to the deliberate acceptance of uncertainty. There is no way of calculating whether such a venture would produce any useful result whatever. But, in the peculiar context of advertising, this need not in itself be an objection. It is clear from the behaviour of advertisers in the matter of appropriation policy that many of them regard the return resulting from advertising expenditure as a matter of uncertainty in any case.

VII

Since the problem of appropriation policy is central to all advertising operations, and since in every case special circumstances surround the problem, the most that a single research inquiry can do is to formulate the problem as precisely as possible, estimate the validity of such common patterns as emerge from existing practice, and indicate further lines of study which may be useful.

The most useful lines for further work appear to be: (1) a statistical description of the incidence of advertising expenditure in different industries and product groups; (2) a series of studies, as numerous as possible, of the relationship between changes in advertising expenditure and changes in sales for particular products in different regions; (3) a series of studies of the relationship between changes in media budgets and changes in sales for particular products at different periods; and (4) an attempt, possibly by specialists in operational research, to discover whether any general guidance can be given concerning decisions on the " marketing mix ", on the composition of media budgets, and on the isolation of the effect of the advertising of particular products from the effects of other selling devices within the control of the advertiser.

The London School of Economics.

Ricardo on Factor Prices and Income Distribution in a Growing Economy[1]

By H. BARKAI

1. One of the fundamental problems considered by the classical economists was that of the causes and consequences of economic growth. Among these consequences particular attention was devoted to the effect of economic growth on the structure of factor prices and thereby on income distribution. This problem was treated in great detail by Ricardo who claimed that " to determine the laws which regulated the distribution . . .of rent, profit and wages in different stages of society . . . is the principal problem in Political Economy ".[2]

The Ricardians claimed that, in a growing economy, the wage rate would remain at the subsistence level, the profit rate would fall, and rent would rise. Income distribution would follow a similar pattern. The shares of both labourers and capitalists would fall, whilst the share of the landlords would rise. These propositions were deduced from Ricardo's model (based on the postulate of diminishing returns) put forward and perfected in the course of the debate on the Corn Laws.

Our purpose in what follows is to re-examine this analysis. We propose first to restate explicitly the postulates of the Ricardian model. We then proceed to deduce from them the relevant theorems on the trend of factor prices and the pattern of income shares. It will be seen that some of these theorems are at variance with those advanced by Ricardo. It will then be shown that these differences arise from the fact that Ricardo's results—on the trend of the shares of the rent bill and of profit income, though not his proposition on the trend of the share of the wage bill—follow from the nature of a *particular* diminishing returns production function which is implicit in the arithmetical example on which he bases his argument. Correspondingly it will be maintained that the analysis of the workings of a general Ricardian model does not warrant definite propositions on the trend of income shares.

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[1] Some sections of this paper form a part of a more comprehensive study of classical economics undertaken at the London School of Economics under the supervision of Professor (now Lord Lionel) Robbins.

The author also wishes to acknowledge gratefully the comments of Dr. E. J. Mishan and Mr. M. H. Peston on several points raised here, and his indebtedness to Professor D. Patinkin for invaluable criticisms of an earlier draft. All responsibility for the views expressed is, of course, the author's own.

[2] D. Ricardo, *Principles of Political Economy*, in D. Ricardo, *Works*, ed. P. Sraffa, Cambridge, 1953, vol. I, p. 5.

2. The fundamental features of the Ricardian system are specified as follows:

(a) A given quantity of land; varying quantities of capital and labour.

(b) A given and *invariable* production function.

(c) A decreasing rate of growth of output as input increases, i.e., diminishing returns.

(d) A constant and institutionally determined wage rate.

To simplify the argument, output in a Ricardian model is usually represented by one commodity only, viz., " wheat ", alternatively denoted as " corn " or as " raw produce ". Similarly, the constant wage rate is defined in terms of this output.

Things are, however, more complicated as far as input is concerned, since both variable inputs, capital and labour, must feature in the analysis. To avoid the necessity of handling three variables at one and the same time, Ricardo (and others among the classical economists) introduced the concept of a " dose " of capital and labour. Thus, input could also be considered as one entity. This obviously presumes constant proportions of the two variable inputs, which inevitably adds another restriction to the model. The production function accordingly involves output as an explicit function of the variable inputs, capital and labour, the latter being composed into uniform doses.

We now proceed to represent in formal terms the relations specified above. Let Y denote output (income), x the number of doses of capital and labour, and w the wage rate defined in units of output. The Ricardian model can then be written as:

$$Y=f(x), \text{ where } f'(x)>0, \text{ and } f''(x)<0 \quad \ldots\ldots\ldots\ldots (1)$$

$$w=w_0 \quad \ldots\ldots\ldots\ldots (2)$$

The production function, (1), relates output (income) to input, according to assumptions (a), (b) and (c) above. Output is an increasing function of capital and labour, increasing at a decreasing rate. The wage relation, (2), specifies constant rates of remuneration corresponding to assumption (d), above.

Let each of the uniform doses of capital and labour be composed of m units of labour per unit of capital. The total reward of the labour component of the dose would hence be mw_0, i.e., the remuneration per unit multiplied by the number of labour units. Now, according to Ricardo, the profit rate is the difference between the return to the marginal dose of capital and labour and the total reward to the labourers composing that dose. Thus writing p for profit rate we have:

$$p=f'(x)-mw_0 \quad \ldots\ldots\ldots\ldots (3)$$

While Ricardo treats the profit rate and the wage rate as variables distinct from the profit income and the wage bill respectively, he treats rent differently. Rent per acre as distinct from total rent is never considered. Rent is accordingly specified as the difference between total product and total rewards to the variable inputs, the latter being equal to the marginal product of a dose of capital and labour *times* the number of doses employed, i.e., $f'(x)x$. Writing R for rent, the expression for rent is:

$$\begin{aligned} R &= Y - f'(x)x \\ &= f(x) - f'(x)x \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4) \end{aligned}$$

The Ricardian theorems on the trend of the profit rate and of rent in a growing system are easily deduced from (3) and (4). Thus, since by assumption $f''(x)$ is negative:

$$\frac{dp}{dx} = f''(x) < 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(5)$$

That is, the rate of profit falls as the inputs of labour and capital increase and output grows. Diminishing returns are similarly the cause of the rise of (total) rent:[1]

$$\frac{dR}{dx} = -f''(x)x > 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(6)$$

3. We turn now to the analysis of the trend of distributional shares. Here in contrast to the results deduced above, our conclusions differ from those stated by Ricardo. Let us first consider wages. The size of the wage bill grows proportionally to that of (labour) input since wage rates are presumed to be constant. Output, however, grows less than proportionally, according to the postulate of diminishing returns. Thus, obviously, the share of wages in income *rises*.

The same results can be derived formally. Write W for the wage bill. Hence we have:

$$W = w_0 mx \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(7)$$

The wage bill as a proportion of income is:

$$\frac{W}{Y} = \frac{w_0 mx}{f(x)};$$

and the rate of change of wages as a proportion of income is:

$$\frac{d}{dx}\left(\frac{W}{Y}\right) = \frac{w_0 m}{[f(x)]^2}\,[f(x) - xf'(x)] > 0 \quad \ldots\ldots\ldots\ldots(8)$$

This must be always positive, since the expression $[f(x) - xf'(x)]$, i.e., *rent*, is positive when more than one dose of labour and capital is being utilized. It can obviously never have a negative value, as negative rent signifies that (total) rewards to variable factors are greater than output, which is impossible in the long run. Nor is zero a feasible value for this expression, since that would mean that total product is wholly absorbed by rewards to capital and labour only, which implies that land is a free factor.

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[1] "However extensive a country may be . . . moderate accumulations of capital, will be attended with great reductions in the rate of profit, and a rapid rise in rent." *Ibid.*, p. 126.

The pattern of rent as a proportion of income can be traced in the same manner. Thus, the share of rent can be expressed as:

$$\frac{R}{Y}=\frac{f(x)-f'(x)x}{f(x)}$$
$$=1-\frac{f'(x)x}{f(x)}.$$

The rate of change of rent as a proportion of income is:

$$\frac{d}{dx}\left(\frac{R}{Y}\right)=-\frac{1}{f(x)}\left\{xf''(x)-f'(x)\left[\frac{f'(x)x}{f(x)}-1\right]\right\}\text{ ....(9)}$$

To simplify presentation we propose to make a somewhat unconventional use of the elasticity concept. Define the elasticity of the production function, e, as:

$$e=\frac{x}{f(x)}f'(x)\text{ ....(10)}$$

Substituting (10) into (9), we have

$$\frac{d}{dx}\left(\frac{R}{Y}\right)=\frac{-1}{f(x)}\left[xf''(x)-f'(x)(e-1)\right]\text{ ....(11)}$$

Now, the elasticity of the production function is always smaller than unity. This is evidently a corollary of the argument presented previously that $[f(x)-xf'(x)]$, i.e., rent, is positive. It follows that the expression $[-f'(x)(e-1)]$ is positive within the range where marginal product $f'(x)$, is positive, i.e., within the relevant range. But $xf''(x)<0$, since by assumption $f''(x)$ is a negative magnitude. Consequently, the sign of the expression in the brackets on the right hand side of (11), and hence of $\frac{d}{dx}\left(\frac{R}{Y}\right)$, cannot be settled on the strength of the available information. This evidently signifies that the trend of rent as a share of income is *indeterminate*.[1]

This conclusion can be further clarified by considering in turn the conditions under which rent as a share of income might rise, fall, or remain constant. Thus, rent would be a rising share if the (combined) reward of capital and labour grows—if it grows at all—at a slower pace than output. This is what presumably happens when returns diminish rapidly, since that involves a strong downward pressure on remuneration per dose of labour and capital. In this case total reward to both factors, being a product of an increasing number of input units and a decreasing remuneration per unit, will grow at a very low rate, which is conceivably lower than that of growth of output. Their combined share in income will hence inevitably decline, and the proportion of rent, the only remaining share, would rise. This holds *a fortiori* if remuneration per dose falls at rates equal to or higher than the rate at which the number of input units grows, so that total reward to capital and labour are either constant or falling.

[1] It is determinate only in the trivial case where $f'(x)=0$.

The postulate of diminishing returns does not imply, however, that marginal product falls at rates of any *particular* order of magnitude.[1] The slope of the returns curve might be numerically small, and could conceivably even decrease. Remunerations per dose of capital and labour would in that case fall slowly, so that the impact of increasing inputs pushing total rewards upwards would be much stronger than the downward pressure effected by the falling remuneration per unit. Rewards to capital and labour would then grow faster than income, so that their (combined) share rises and that of rent falls.

A third possibility is also conceivable. In this case the falling rates of remuneration are countervailed by rising inputs, the latter growing at just such rates which make for an equiproportional growth of rewards to capital and labour and of income. Their share, and hence that of rent, in total income consequently remain constant.

Consider now profit income as a share of income. Writing P for profit income, we have:

$$P = px \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (12)$$

and

$$\frac{P}{Y} = \frac{[f'(x) - mw_0]x}{f(x)}.$$

Now,

$$\frac{d}{dx}\left(\frac{P}{Y}\right) = \frac{1}{f(x)}\left\{xf''(x) - [f'(x) - mw_0]\,[e-1]\right\} \quad \ldots . (13)$$

It is evident that (13), which indicates how the share of profit varies, is similar to (11), which expresses the trend of rent as a share of income. The only difference is in the factor mw_0—total rewards of the labour component of the dose—which is a constant. Since $[f'(x) - mw_0]$, the profit rate, is a positive magnitude in the relevant range, $\frac{d}{dx}\left(\frac{P}{Y}\right)$ may, similarly to $\frac{d}{dx}\left(\frac{R}{Y}\right)$, go either way. Thus the trend of profit as a share of income also is *indeterminate.*

This result could also be derived from the argument, above, which shows that the (combined) reward to capital and labour might grow faster than income. It is therefore conceivable that the share of profit

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[1] Statements in the literature relating to the " law of diminishing returns " and its corollaries are occasionally not quite clear on this point. They sometimes give the impression that the diminishing returns postulate implies that returns diminish at an increasing rate. This notion is a misconception. The law of diminishing returns only states that marginal product diminishes as inputs are increased. It does not refer at all to the *rate* at which marginal product diminishes. Any of the three alternatives of constant, increasing, or decreasing rates is consistent with the law.

In terms of the notation adopted in this paper, the law of diminishing returns is expressed by the negative sign of the second derivative of the production function $f''(x) < 0$. A statement on rates of change in returns requires a specification of the third derivative of the function, i.e., $f'''(x)$, which is evidently an additional restriction, and not a corollary of the former.

(being a component of the former) in income would rise on its own, leaving rent as the only falling share. The share of the wage bill, the second component in the reward to capital and labour, always rises.[1] On the other hand, if the share of the combined reward of capital and labour falls, the increasing share of wages inevitably involves a fall in that of profits.

We may consequently conclude that, contrary to what was commonly accepted, the postulate of diminishing returns is insufficient for the derivation of a general theorem on the pattern of income shares in a growing economy. The only generally valid theorem applies to the share of wages. In a Ricardian model of the type specified above, the share of labour in total income rises, but the trend of the shares of the other two factors of production is indeterminate.

4. Ricardo, however, was convinced that the analysis of the workings of his model leads to the definite conclusion that in proportion to the progress of wealth and the difficulty of producing food, although the value of the labourer's portion will be increased, his *real share will be diminished*, whilst that of the landlord will not only be raised in value but *will also be increased in quantity*. The remaining quantity of the produce of the land constitutes the profit of stock . . . as society advances, *this proportion* of the whole produce *will be diminished*.[2] The proof of this theorem consists of an arithmetical example spread over three chapters of his *Principles*. Its first part is introduced as a concluding footnote to the chapter on rent, and it is later expanded in the chapters on wages and profits.

The model outlined earlier in this paper is on the whole an analytical restatement of Ricardo's model implicit in his numerical example. The only difference between the two is the implicit assumption of an additional restriction, that of a *linear* diminishing returns function, in the numerical example; this takes the place of the general diminishing returns function used in the model specified above. The restrictive assumption is apparent in his returns schedule (cf. Table I below), which yields a function

$$\frac{dY}{dx} = -10x + 190,$$

for which we may substitute the general linear expression

$$\frac{dY}{dx} = b - ax.$$

The production (income) function is accordingly—

$$Y = \int (b - ax)\,dx = bx - \frac{ax^2}{2} + c.$$

Since no output would be produced when variable inputs are zero, put $c=0$, so that Ricardo's data yields a parabolic production (income) function

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[1] *Cf.* p. 242 above.

[2] *Ibid.*, p. 112.

$$Y = \frac{2bx - ax^2}{2} \quad \text{.......................}(14)$$

By the first condition attached to (1)—a positive marginal product, $f'(x) > 0$—the relevant range of the independent variable x is

$$0 < x < \frac{b}{a} \quad \text{.......................}(15)$$

The additional restriction provided by the linearity of the diminishing marginal product function is unnecessary to deduce the theorems on the trend of the profit *rate* and rent, and of wages as a proportion of total product, i.e., results (5), (6) and (8), above. The linearity of the marginal product function is, however, of crucial importance for the deduction of Ricardo's proposition on *determinate* trends in the shares of rent and profit in total income. Thus if we substitute (14) and condition (15) into (11), we get a definite result:

$$\left(\frac{d}{dx}\right)\frac{R}{Y} > 0 \quad \text{.......................}(16)$$

Rent is a rising share of total income when capital, labour and output expand. Its corollary,

$$\frac{d}{dx}\left(\frac{P}{Y}\right) < 0, \text{.......................}(17)$$

follows at once. Profits are a falling share of total income. Several other simple marginal product functions, for instance a rectangular hyperbola, yield similar results to that of the linear case.

In short, Ricardo's theorem on distribution is not *general* in the sense that it holds for *any* system in which production is subject to diminishing returns, as he presumably believed. It holds only if marginal products diminish according to some *definite* laws.

5. We have seen that the difference between our results and the orthodox Ricardian ones can be attributed to a difference in the underlying set of assumptions. This difference does not, however, explain the contradiction in the theorems on the trend of the share of wages; for in both cases considered above (with a general and a linear diminishing returns function respectively), our conclusions on the trend of wages are identical: the share of wages *rises*. Ricardo, however, asserts that that share *falls*, and claims to have proved that proposition in his arithmetical example with a linear diminishing returns function. To trace the cause of this contradiction we propose to restate the example and analyse the proof.

Ricardo first specifies a diminishing returns schedule for wheat, assumed to represent conditions of agricultural production in the economy as a whole. Returns in the non-agricultural sector are tacitly supposed to be constant.[1] He next defines the conventional minimum " subsistence " in terms of a basket consisting of two commodities, wheat and a comprehensive commodity denoted as " other things ".[2] This is said to be equivalent to $3q$ of wheat and a quantity of " other things ", the value of which in terms of the former is $3q$, making $6q$ altogether. The money price of wheat is then specified at £4 per q at the outset. Money is assumed to be an invariable measure of value throughout.

When after the initial dose, additional doses of input are set to work, it is shown how, in consequence of the lower returns, the price of wheat rises proportionally. This involves a higher money wage rate, in accordance with the assumption of minimum subsistence defined in terms of the basket. But since the production costs of " other things ", the second item in the basket, do not rise, money wages rise as the price of wheat rises, but less than proportionally. The information may be summarized as in the following table.

TABLE I

INPUTS, RETURNS, PRICES AND MONEY WAGES

1	2	3	4	5	6
Input doses	Marginal product in q	Money price of wheat per q	Value of food component of subsistence basket	Value of "other things" component of subsistence basket	Money wages
		£ s. d.	£ s. d.	£ s. d.	£ s. d.
1	180	4 0 0	12 0 0	12 0 0	24 0 0
2	170	4 4 8	12 14 0	12 0 0	24 14 0
3	160	4 10 0	13 10 0	12 0 0	25 10 0
4	150	4 16 0	14 8 0	12 0 0	26 8 0
5	140	5 2 10	15 8 6	12 0 0	27 8 6

NOTES

Columns 1 and 2 incorporate Ricardo's returns schedule.

The money price per q in column 3 is calculated by multiplying the ratio of the initial marginal product to the respective marginal products by £4, the " original " price of wheat. Thus, when marginal product is 170, price would be £4 $\times \frac{180}{170}$ = £4 4s. 8d., etc.

Column 4 is calculated by multiplying the food component of the subsistence basket ($3q$) by the corresponding price of food. The entries in column 5 are constant throughout, since productivity in the non-agricultural sphere is assumed not to change. The entries in column 6 are the sum of the corresponding entries in columns 4 and 5.

By dividing *money* wages by the respective price of wheat, it is easily shown that wages in terms of *wheat* fall as more labour is employed and as output increases. From this data and the returns schedule, profits and rent in terms of wheat are calculated as follows:

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[1] *Ibid.*, pp. 83 and 103.

[2] *Ibid.*, p. 103.

TABLE II

DISTRIBUTIONAL SHARES IN REAL TERMS AND AS PERCENTAGES OF TOTAL INCOME

1	2	3	4	5	6	7	8
		Wheat wages		Wheat profits		Wheat rents	
Input doses	Marginal product in *q*	in *q*	as % of income	in *q*	as % of income	in *q*	as % of income
1	180	60	33·3	120	66·6	—	—
2	170	58·3	32·3	111·7	62·2	10	5·5
3	160	56·6	31·4	103·4	58·6	20	9·9
4	150	55	30·1	95	52·9	30	16·8
5	140	53·3	29·8	86·7	49·2	40	19·8

NOTES

Wheat wages are calculated by dividing the entries in column 6 of Table I by the corresponding entries in column 3 in the same Table, and multiplying the quotient by ten, the number of labourers in the standard dose.

Wheat profits are derived by subtracting the corresponding entries in column 3 from those in column 2 in Table II.

The entries for rent are calculated by subtracting the respective marginal product from the marginal product of the first dose, which is 180*q*. of wheat.

Percentages are calculated, throughout, on the basis of 180*q*.

The first impression one gets from a reading of Table II is the perceptible falling trend of (wheat) wages and of profits and the rising trend of rent. The percentage columns, which are not given in Ricardo's original but are calculated (following up his hint) on the basis of 180*q* throughout,[1] *prima facie* fortify Ricardo's conclusion on the trend of proportional shares; the shares of both "productive" classes, capitalists and labourers, fall, whilst those of landlords rise.[2]

There are, however, two flaws in the argument. First of all, the results do not apply to the problem Ricardo set out to solve. This fact is glaringly manifest in Table II. By reading across any one of the five rows, we find that the sum of the shares distributed to the three factors always is 180*q*. This is the marginal and also the total product of the *first* dose of labour. It is, however, certainly more than the marginal product and less than the total product when the input is more than one dose, as the entries in rows 2 to 5 indicate. Consequently, the table does not show what it is supposed to do, viz., how "in different stages of society, the *proportions* of the *whole* produce of earth are allotted . . . under the names of rent, profit and wages".[3]

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[1] *Ibid.*, p. 116.

[2] For the examples and numerical data in Tables I and II and the related set of postulates, cf. *Principles*, pp. 83, 102–3 and 115–7. Table II is essentially a reproduction of the third table on p. 116. The percentage columns have been added.

[3] *Ibid.*, p. 5.

The only warranted interpretation of Ricardo's table (Table II above) is that it portrays the variation with increases in inputs (and hence in outputs) of the distribution of the product *of the first dose* (180q) applied to production. This is something quite different from the pattern of income distribution of a *growing* national income (output).

Yet even if we interpret Ricardo's theorem in the sense suggested above, his numerical data and his result on the trend of the share of wages remain unwarranted. This is so because his argument is based on the calculation of wheat wages, and this yields neither real wages nor real profit and rent in terms of the " subsistence " basket as defined by himself. Money wages, as can easily be seen in Table I, are always sufficient to buy 3q of wheat and exactly the same quantity of the comprehensive commodity " other things ". This holds by definition, since, consistent with his assumption of a constant " subsistence " real wage rate, Ricardo justifiably calculates money wages by adding the value of " other things " (at their postulated constant money price) to the value of the same quantity of wheat, the money price of which varies because of diminishing returns. Thus, wages in terms of wheat necessarily fall, but wages in terms of the basket are *constant* throughout.

Yet, by implicitly identifying wheat wages with the " real share " of labourers, a procedure which was almost subconsciously implanted in classical thought, Ricardo came to the fallacious conclusion that the share of wages falls as the system moves along a rising trend. What led him astray on this point was his definition of real wages in terms of a basket composed of two commodities, and later his identification of the same variable with only one component of the former. Unfortunately, neither he nor his followers realised that this identification was inconsistent with the original definition.

Had Ricardo avoided this pitfall and, as consistency requires, had he defined his subsistence basket in the same terms as output, i.e., in terms of wheat only, his calculation would have yielded different results. Column 3 of Table II would not have shown falling real wages and he could not have claimed that growth involves a fall in the share of wages. Yet even this on its own would not have settled the matter, since his proof applies, as we have seen, to the modifications in the pattern of distribution of the product of the *first* dose only, and not to that of total product.

His data—the returns schedule and the specification of the real wage rate—are nevertheless sufficient for the derivation of both a theorem on the trend of factor prices (other than the postulated constant wage rate) and a theorem on the trend of income distribution. This is what we have done in section 4 above, where we have set out Ricardo's example, in its most general form, as the *linear* case. The same could have been achieved by applying an arithmetical method akin to that employed by Ricardo, which, consistently used, would have yielded an identical result on the trend of wages: the share of income taken by the wage bill rises as input increases and output grows.[1]

6. The most significant result of our enquiry is then that the general Ricardian model, presuming only diminishing returns and constant wage rates, does not provide for a general theorem on the trend of distribution. The post-Ricardian classical economists nevertheless believed that Ricardo had succeeded in establishing that very theorem, and so had solved one of the most important problems of Political Economy. Hence the tremendous impact on opinion, and particularly on Marx and on Socialist thought in general, of the Ricardian theorems on factor prices and on distribution. It was not realized, in particular, that Ricardo's proposition on the trend of the share of wages is, on his own premises, a mistake. Neither was the nature of the postulated framework fully comprehended, and hence also the restriction it imposes on the applicability of these theorems to reality. Yet the inherent weakness of the Ricardian model in this respect manifests itself immediately, once the nature of the conceptual framework is made explicit by the specification of its basic set of premises. It should be obvious that the Ricardian theorems hinge on the postulated invariability of production functions (assumption (b) above). The Ricardian theory on factor prices and distribution, set out in sections 3 and 4 above, thus excluded technical change from the list of factors bearing upon the pattern of factor remuneration and of income distribution. This necessarily restricted severely its significance, and made it practically useless as a guide for policy and as a basis for forecasting the secular trend of economies, such as countries in Western Europe in the post-Ricardian period, where rapid transformations in technology were an inherent phenomenon of the economic process.

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[1] From the data in the marginal product column (column 2) of Table II, a total product column could easily be calculated. This would be (in q of wheat)—180, 350, 510, 640, 780, corresponding to the ascending number of input doses and the respective marginal products. Assuming 10 labourers per dose and a subsistence wage rate of $6q$, the corresponding column of the wage bill would be (in q of wheat)—60, 120, 180, 240, 300. This obviously yields a rising share of the wage bill in income.

The profit and rent columns could be similarly calçulated from the respective entries of the marginal product and total product columns, and would show a rise in the share of rent and a fall in the share of profit.

Stock and Flow Quantities: A Common Fallacy

By R. W. Clower

Confusion between symbols and the "things" to which symbols refer is fairly common in every science. In most cases, loose argument of this kind is analytically innocuous and may be permitted to pass without comment. When a well known economist asserts that a change in a stock quantity has the dimensions of a flow, however, and proceeds to criticize the work of other writers on the basis of his own confusion,[1] a rebuke is in order before the fallacy gains a secure foothold in the literature.

According to Patinkin, "... the excess demand for money as a *stock* has the dimensions of a *flow*. For it is the difference between the stock of money at two points of time ...".[2] The nature of the fallacy is best seen by going to its source. Thus, if $x(t)$ refers to the "stock" of a given commodity at the beginning of period t, the "flow" of the same commodity during period t is then represented by

$$\frac{\Delta x(t)}{\Delta t} = \frac{x(t+1)-x(t)}{t+1-t} \equiv x(t+1)-x(t). \quad \ldots\ldots\ldots\ldots(1)$$

But $x(t+1)-x(t)$ is the difference between stocks in period $t+1$ and stocks in period t, so (1) appears to imply that the difference between the total stock of a given commodity at two points of time has the dimensions of a flow—which is precisely what Professor Patinkin asserts. In fact, however (1) implies nothing of the sort.

The correct inference from (1) is this: The *number* which measures the difference between the total stock of a given commodity at two given points of time also happens in this case to measure the flow of the commodity during the interval of time defined by the given points. Thus, a single (numerical) symbol suffices to characterize a property of two distinct "things"—a "stock" in the first case, a "flow" in the second. But this does not mean that the two "things" referred to are identical; they are not.

The fallacy probably has its origin in the tacit assumption (almost universally adopted in economics) that $\Delta t = 1$. If the more general assumption $\Delta t = h \neq 1$ is adopted, then

$$\frac{\Delta x(t)}{\Delta t} = \frac{x(t+h)-x(t)}{t+h-t} \equiv \frac{x(t+h)-x(t)}{h},$$

which is clearly *not* equal to $x(t+h)-x(t)$. No confusion arises in this case.

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[1] Don Patinkin, "Liquidity Preference and Loanable Funds: Stock and Flow Analysis", *Economica*, November 1958, pp. 300–318.

[2] *Ibid.*, p. 303. See also Patinkin's *Money, Interest and Prices*, 1956, p. 25, n. 17, and p. 147, n. 16.

The implications of the above remarks for Patinkin's *ex cathedra* comments on stock and flow analysis (pp. 305–7 of his article) are fairly obvious and will not be elaborated here. I will only venture the suggestion that anyone who proposes to do research in stock-flow analysis will do well to reconsider the "folklore" of the subject carefully before making use of any of it in his work. The fallacy discussed here is merely one of many similar confusions in this branch of the economic literature.

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The Rate of Interest and Walras' Law

By H. Rose

In the course of a recent article[1] Patinkin produces an argument which purports to destroy a position which I have upheld elsewhere.[2] That position was as follows:

(1) The difference between the liquidity preference (*LP*) and loanable funds (*LF*) theories of interest lies in the dynamic assumptions they make.

(2) On the assumption of a production lag but no consumption or dividend lags, the contending dynamic hypotheses are:

LF: When the sum of the excess demands for goods and money is positive, the rate of interest rises, and when the sum is negative, the rate falls, and

LP: When there is an excess demand for money, the rate of interest rises, and when there is an excess supply, it falls.

(3) *Both* theories assert that the rate of interest rises when there is an excess demand for loans, and falls when there is an excess supply.

The implication of (2) and (3) is that *LF* identifies the excess demand for loans with the sum of the excess demands for goods and money, while *LP* identifies it with the excess demand for money only. Since the former identity is simply Walras' Law, it follows that *LF* asserts, and *LP* denies, the validity of Walras' Law.

The Keynesian reason for rejecting the Law is connected with the behaviour in the loan market of firms which invest passively in stocks of goods. They are supposed to finance such stock movements (and to retire surplus finance when their stocks run down) by credit transactions in the period in which the movements occur. Passive investment has therefore to be included in the excess demand for loans.

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[1] D. Patinkin, "Liquidity Preference and Loanable Funds: Stock and Flow Analysis", *Economica*, November 1958, pp. 300–318.

[2] H. Rose, "Liquidity Preference and Loanable Funds", *Review of Economic Studies*, February, 1957, pp. 111–119.

But passive investment is identical with the excess supply of goods. Consequently the excess demand for loans consists of the excess demand for goods *plus* the excess demand for money *plus* the excess supply of goods, and is therefore identical with the excess demand for money. Walras' Law, on the other hand, by omitting passive investment from the demand for loans, implies that the passive investors " finance " their investment currently by an unplanned disbursement of cash, and only subsequently enter the loan market to restore their cash position.

Patinkin has no difficulty in proving that if one maintains (*a*) that movements of the rate of interest depend exclusively on the excess demand for money, (*b*) that the excess demand for loans is the same as the excess demand for money, and (*c*) that Walras' Law holds, the result is a nonsense. He concludes that, *since* (*c*) *is true*, one cannot maintain both (*a*) and (*b*), and that (*a*) is implausible without (*b*).

If, however, one contends, as I think a Keynesian must, that (*a*) and (*b*) are true and therefore (*c*) is false, Patinkin's difficulties do not arise. Specifically, it is not true that the excess demand for goods (excess of demand over production) is identically zero,[1] nor is it the case that one can describe the movements of the economy in terms of any two of the three excess demands.[2] It is illegitimate to omit the excess demand for goods. Patinkin's indeterminacy argument[3] is therefore irrelevant.

Exeter.

Reply to R. W. Clower and H. Rose

By Don Patinkin

I

The confusion with which Mr. Clower charges me is a serious one. And, to cut things short, I will readily admit that there are some loose sentences in my article which—if torn out of context—can be made to appear as confused as Mr. Clower has made them appear. From this I deduce two things: first, that one should not write loose sentences; second, that one should not tear them out of context.

Mr. Clower notwithstanding, it was not my intention to argue that the difference between a stock at two different points of time has the dimensions of a flow, but rather that the change effected (or to be effected) in a stock *over a given period of time* has the dimensions of a flow. Though the sentence which Mr. Clower has chosen to cite is admittedly not clear on this point, its real intention should have been clear to him from the sentence which appears only two paragraphs later and which states explicitly that " an individual can be satisfied with the level of his planned stock at the end of the week if, and only if, he is satisfied with his planned *net inflow during the week* ".[1]

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[1] Patinkin, *op. cit.*, p. 313.

[2] *Ibid.*, p. 312.

[3] *Ibid.*

In mathematical terms, let $C(t)$ represent the stock at time t. Then the magnitude which is discussed in my article is *not*

$$C(t) - C(t-1) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

as Mr. Clower would have it, but

$$\frac{C(t) - C(t-1)}{t-(t-1)} = \frac{C(t) - C(t-1)}{1} = C(t) - C(t-1) \quad \ldots\ldots (2)$$

per period of time, where the " 1 " in the denominator of the second term has the dimensions of time. The magnitude in (2) clearly has the dimensions of a flow; but the magnitude in (1)—as Mr. Clower has so rightly emphasized—has the dimensions of a stock.[2]

Though I am happy for having had this opportunity of clarifying these ambiguities in my original article, I trust that it is clear from the above that there is really no disagreement between Mr. Clower and myself on the question raised by him.

II

Mr. Rose's comment relates to a point which is quite incidental to my main argument. Furthermore, it is based on an uncritical application of one of Bent Hansen's remarks about the nature of Walras' Law.[3] The essence of these remarks is that if the demand for loans (or any other good) is defined in the (unusual) *ex post* sense, while the demand for money is defined in the (usual) *ex ante* sense, then the excess demand for money is *not* equal to the value of the

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[1] " Liquidity Preference and Loanable Funds ", *loc. cit.*, p. 304, italics added. In this connection, the reader will find it instructive to consult the whole paragraph from which this sentence is taken—as well as the paragraph beginning, " Before concluding . . . ", on p. 306.

[2] Note that corresponding statements can be made in terms of time integrals. Namely, my discussion is *not* related to

$$\int_0^t f(t)dt - \int_0^{t-1} f(t)dt$$

(where $f(t)$ is the flow of a certain quantity), but to

$$\frac{\int_0^t f(t)dt - \int_0^{t-1} f(t)dt}{\int_{t-1}^t dt} = \frac{\int_0^t f(t)dt - \int_0^{t-1} f(t)dt}{1}.$$

The first of these expressions has the dimensions of a stock, while the second has, of course, those of a flow.

[3] Cf. the reference to Bent Hansen's *Study in the Theory of Inflation* (London, 1951), pp. 194–197, on p. 113, footnote 5 of Mr. Rose's original article.

excess supply of other goods. This is certainly not surprising. But I do have difficulty in understanding the meaning and significance of an argument which mixes *ex ante* and *ex post* quantities in this way.[1]

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[1] What Mr. Rose may have in mind—though I am not sure—is the effect of intermarket pressures on the dynamic time paths of the variables (cf. my *Money, Interest and Prices*, pp. 157–158, 179 and 266–267). But this, of course, has nothing to do with Walras' Law.

" Coles " : Weights and Measures, with Special Reference to Abraham Darby and the Coke-smelting Process

By R. A. Mott

M. W. Flinn[1] has questioned my conclusion[2] that coke, made from coal, was the only fuel used by Abraham Darby I in the Old Blast Furnace at Coalbrookdale in 1709, on the grounds that the word " coles " at that period meant charcoal and that the contemporary writer would almost certainly have used the expression " pit-coal ", " stone-coal " or " sea coal " when mineral coal was intended. To suggest that examples of usage in the Furness peninsula and Sussex (mineral coal being found in neither area) might apply to the coalfields is, to say the least, rather naïve.

The earliest European words for solid fuel—at a time when the predominating fuel was what we now call charcoal—were the Greek *anthrax* (pl. *anthraces*) and the Latin *carbo* (pl. *carbones*). Since Latin was the official legal language in England from the Norman conquest until the 14th century, *carbones* would be used for fuel; the English equivalent in literary usage was, apparently, " cole ",[3] though the earliest known usage in relation to iron-making is " charcole ".[4]

Charcoal, like corn, was measured in units of capacity. A practical measure of charcoal used at the Tudely bloomery, Kent, in 1332, and at the Bishop of Durham's bloomery at Kyrkenott, Weardale, in 1408, was the *duodena*, i.e., a dozen quarters,[5] a quarter being a light (120 lb. of charcoal, modern measure) though awkward (8 cu. ft.) load for a pony to carry in two " banisters " (panniers). This method of transport was unsuitable on the heavy clay of Sussex, where, in the

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[1] M. W. Flinn, " Abraham Darby and the Coke-smelting Process ", *Economica*, vol. XXVI (1959), p. 54.

[2] R. A. Mott, " The Earliest Use of Coke for Iron Making ", *Gas World* (Coking Section), vol. 50 (1957), p. 7; and paper read to Newcomen Society, 11th December, 1957.

[3] R. Rolle (1290 ?–1349) of Hampole, *Prick of Conscience*, (1340) (line 6762): " Thair hertes sal bryn within als a cole ", a usage probably dependent on the biblical " Thou shalt heap coals of fire on his head " (Romans, xii, 20).

[4] M. S. Giuseppi, *Archaeologia*, vol. LXIV (1912–13), p. 145: 1330, " bosco mortuis . . . pro carbonibus faciendis dictus blomos "; 1332, " carbonibus (em)ptis qualibit decena continenti xxiiij (quarteria) carbonum "; 1333–4, " xx duodenis carbonem ad dictos blom ' ".

Public Record Office Document (Durham, Auditors' Vouchers; box: Durham 20/114): 17 Nov., 10 Henry IV, " quatuor duodenas carbonis vocatur charcole "; document transcribed by G. T. Lapsley, *Economic History Review*, vol. 14 (1899), p. 509, but note that he, like most other writers, incorrectly referred to the site as " Byrkenott ".

[5] See preceding footnote.

16th century, the dozen quarters were carried in a wain: this " load " became the common unit, though it usually retained the name " dozen "; the bushel concerned was that introduced in 1497 (1.241 cu. ft. in modern measure), so that the load or dozen of charcoal was 0.8 ton.

When, with Magna Carta (1215) and the Forest Charter of Henry III (1217), the severity of the early Norman rule was relaxed, rights by common law to dig mineral coal were established. This new form of coles was given a special name—sea-coals—to distinguish it from the usual form. In the grants of the 13th century which refer to the mining of mineral fuel in, for example, Northumberland, the Forest of Dean (Glos.), Sedgley, Walsall and Tunstall (Staffs.) and Colne (Lancs.), the common form is *putei de carbone maris*[1] (pits of sea-coals).

In the reign of Henry III, sea-coals arriving at Billingsgate was assessed by the King's quarter, but the chalder (=4 quarters)[2] became a more usual measure than the dozen, for it also represented a convenient cart-load. Eventually, the London chaldron for the measurement of sea-coals became 4 quarters, each of 9 bushels, and in 1595, the Newcastle chaldron was equal to 1½ London chaldrons.[3] However, chaldrons as a measure of mineral coal were confined mostly to the Newcastle–London trade.

The original chalder of 32 bushels (32 cu. ft.) was one-quarter of the statute measure for a cord of wood (128 cu. ft.). The large coal of many of the inland pits could conveniently be stacked to await transport, and the " stack " of the Coalbrookdale coalfield was 3 cu. yd., equivalent to 35 cwt.[4] A similar unit was used in the Nottingham and Derby areas.[5]

In the century before William of Orange became King, the production of mineral coal in Britain increased 14-fold,[6] and even in the reign of the first Stuart it was said:[7] " sea cole and pitt cole is become the general fuell of this Brittaine island ". Under such conditions it was inevitable that " coles " would come to mean mineral coal, particularly in the coalfields.[1] Nef[2] estimated that the output of the Shropshire coalfield increased from about 6,000 tons *p.a.* before the accession of Elizabeth I to about 150,000 tons *p.a.* at the end of the 17th century, and stated that " Coalbrookdale was probably more developed than any Midland mining district during the seventeenth " century; the increase was 25-fold and the latter quantity is 15 times the weight of charcoal required by the six charcoal furnaces and fifteen forges in Shropshire *ca.* 1720.[3] It is clear that, to the people of Shropshire at the end of the 17th century, " coles " must usually have meant mineral coal.

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[1] R. L. Galloway, *Annals of Coal Mining and the Coal Trade* (1st series), London, 1898, p. 26 *et seq.*

[2] York Fabric Rolls, probably 1370–80: " Et in xiij Chaudres carbonum marinorum emptis pro. toral calc. cum. portagio iiij li xvis ixd ".

[3] J. U. Nef, *The Rise of the British Coal Industry*, London, 1932, vol. II, App. C.

[4] Exch. Dep. Com., 27 Geo. II/Hil 8 (Public Record Office, Document E 134), evidence of G. Perry: The ironworks at Coalbrookdale in 1726–42 consumed, *p.a.*, an average of 3,700 stacks of coal " reckoning thirty five hundred weights of coals to each stack ".

[5] Historical Manuscripts Commission, *Calendar of the Manuscripts of the Duke of Rutland*, London, 1891, vol. I, p. 220, referring to the year 1587: " A rooke of colles ought to be ij yeardes high and a yeard and a quarter square " (i.e. 84·5 cu. ft.).

J. Houghton, *Husbandry and Trade Improv'd*, London, 1727, vol. I, letter dated 22nd April, 1693: " John Lowe of Denby, Esq. . . . had got above one thousand loads upon the bank, any one of which he upholds to be at least five and thirty hundred weight, and sells them for five shillings sixpence, so that this year we bought coal ".

[6] Nef., *op. cit.*, vol. I, p. 20 *et seq.*

[7] E. Howes, in Stowe's *Annales*, London, 1615, p. 210.

In the years 1718–27 (when Flinn concedes that mineral coal was used at Coalbrookdale), the plural word " coals " was always used when listing the quantities of " Big Coals ", supplied only by Richard Hartshorne, in units of stacks and loads[4] (1 stack = 9 loads, each horse-load being of just under 4 cwt.). From December, 1720, charcoal was obtained for the Middle Forge, the units being dozens and sacks (1 dozen = 12 sacks); in 16 references in the form " Coleing . . . Doz Cole " (or " Coal ", " Charcole ", or " Charcoal "), the plural form was used only twice.[5] In the 1709 accounts, on the other hand, the word " Coles " occurs 14 times, the singular " coal " only once;[6] and the implication that the " Coles " of 1709 was mineral coal is confirmed by certain entries which give the units:

1709, 30 April to Coles Reseved by Roger Cock from Dorrells pit
9 stack 7 loads, 1 dozen & 5 loads of lumps
2 May moore by Cock 2 stk 3 loads & 1 dozen 7 loads lumps

Moreover, there is the last (and uncompleted) entry in the accounts:

1710, 7 Jan. Rd. Hartshorn & Company Cr
By Stack Loads of Coal
By Doz Loads of Lumps

Lump coal was bought for roasting the ironstone and only accounted in later records for about four per cent. of the total, which was therefore mainly " Big ", all of this being coked.

There can, therefore, be no doubt that the coles supplied in 1709 was mineral coal. There are other problems of interpretation with which the writer has dealt elsewhere,[1] or will be dealing in his paper " Coalbrookdale: 1709 ", to be presented at the meeting which is to be held in September, 1959, to commemorate the 250th anniversary of the first successful use of coke in iron-making. There need be no fear that this commemoration is premature.

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[1] R. Plot, *The Natural History of Staffordshire*, Oxford, 1686, p. 127: " Kitchin fires which are much better supply'd by the common *coal* of the country, especially that of *Wednesbury*, *Dudley* and *Sedgley* "; and p. 167: " they also heat their furnace for a weeks time with charcoal without blowing it ".

Fairhurst MSS., referring to William, Earl of Pembroke's manor of Kimberworth, Rotherham, 1628: " Profitts of Coalepitts, there val. per annum de claro L libri whereof delivered to the forge coales yerely about 8li 12s " (an early indication of the use of mineral coal in the chafery).

F. Brokesby, *A letter . . . containing an account of some observations relating to the antiquities and natural history of England*, referring to the year 1711: " At Madeley . . . adjoining the Severn are considerable cole mines into which they descend not, as in other places, into pitts, but go in at the side of a hill, into which are long passages, both straight forward, and from there at each side; from whence they have dug the coles "; see J. Leland, *Itinerary*, 1744–5, vol. 6.

[2] Nef., *op. cit.*, vol. I, p. 65.

[3] R. A. Mott, paper read to Newcomen Society, 11th December, 1957.

[4] Coalbrookdale Co., Stock Book, 1718–27 (Shrewsbury Library MS. 330).

[5] Coalbrookdale Co., Cash Book, No. 1, 1718–32 (Shrewsbury Library MS. 329).

[6] Coalbrookdale Co. Accounts, 1708–9 (Shrewsbury Library MS. 328).

Chesterfield.

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[1] R. A. Mott, paper read to Shropshire Archæological Society, 17th May, 1958, to be published in the Society's *Transactions*, vol. LVI (part I), and further paper to be published in the same journal.

Book Reviews

The Long View and the Short. By JACOB VINER. The Free Press, Glencoe, Illinois. George Allen & Unwin Ltd. 1958. viii + 462 pp. 42s.

Professor Viner's " students and other friends " have done well to celebrate his birthday by bringing his own miscellaneous writings together into a volume—better than they could have done by asking distinguished persons to contribute some pieces in honour of him. For I am confident that this volume will, by bringing into one view a variety of endeavours, enhance Professor Viner's reputation, already high; it will establish him as the foremost economist of our age for his combination of scholarship and judgment. And it will establish him also as a very distinguished master of English prose, for which the occasional piece gives better opportunity than the prolonged systematic attack upon a particular problem, theoretical or statistical. He can enter, in particular, into the great tradition of economists' prose, through being able to construct sentences which carry in their bosoms qualifications that it is most needful to bear in mind, and yet remain in their totality crisp and decisive. Having regard to the present condition, and, perhaps, to the essential nature, of economics, ability to do this may be necessary for illuminating a subject; a precisely worded excursus, based on rigid " assumptions " set out in a preamble, often sows false impressions as it goes; and it may be impossible subsequently to eradicate the weeds from the reader's mind. Professor Viner's brilliant wit shines forth too.

There is an essay on Schumpeter's *History of Economic Analysis* in which one great scholar confronts another. Viner may not have read so many economists as Schumpeter—I do not know. But Viner's superiority of judgment so clearly outweighs Schumpeter's superiority of erudition, that on the *combination* Viner is surely the master. And even in erudition Viner competes, because he has so much collateral erudition in the fields of philosophy and literature. This essay, very generous in its tributes to Schumpeter, displays also his great literary gift, for rarely can so many relevant considerations have been packed together, without any sense of overcrowding, into a given space.

> " All that I plead on behalf of scholarship, at least upon this occasion, is that, once the taste for it has been aroused, it gives a sense of largeness even to one's small quests, and a sense of fullness even to the small answers to problems large or small which it yields, a sense which can never in any other way be attained, for which no other source of human gratification can, to the addict, be a satisfying substitute, which gains instead of loses in quality and quantity and in pleasure-yielding capacity by being shared with others—and which, unlike golf, improves with age ".

Readers should be warned that Part I of this book is less intensely interesting than what follows. The former contains essays in theory from 1921 to 1950; in some cases the theory has, through Viner himself and others, become well established; in others it is, if not outmoded, at least provocative of the tiresome question—" Well, but how would you wish to modify this in the light of subsequent developments, theoretical or factual ?" The later "parts" call for no such modification. Among other good things, we find full length accounts of the thought of Adam Smith, Bentham, J. S. Mill and Marshall, where Viner's fine judgment and spicy common sense have contrived to convey new and important truths about these masters.

The volume is named, with a transposition, after an impressive address, entitled *The Short View and the Long in Economic Policy*. Traditionally, Viner urges, economists have pressed upon public attention the long view (1939). This statesmanlike pronouncement follows hard upon a review of Keynes' *General Theory* (1936), where, having paid his tribute, Viner makes well-worded criticisms of a number of Keynes' central positions, accusing him *en passant* of gross exaggeration. The sheer weightiness of Viner may give pause even to a hardened Keynesian, like myself. Was Keynes too much addicted to the short period ? Did he exaggerate ? But the pause is momentary only. A large part of the mental activity of an economist must surely be in trying to interpret what is currently proceeding according to his lights. For me, as such an interpreter, the Keynesian "revolution" of concepts provided a vast sense of emancipation and enlightenment; through it I felt able to understand the progress of events—imperfect though my understanding doubtless remained—far better in the 'thirties than I could in the 'twenties. No amount of argument can alter that fact for me. And I cannot help expressing regret that the extraordinarily subtle and penetrating mind of Viner experienced no similar enlightenment—at least so I judge the matter. Time has moved on, and in the last decade I have found an even more potent source of illumination, in watching current events, from the concepts of dynamic economics. But of Keynes it must be remembered that his concepts provided the essential link between the old traditional theory and modern dynamics.

There is in this collection a review (1950) of a report for E.C.O.S.O.C. by Professor J. M. Clark and others on national and international measures for full employment, which brought a "slight shudder" to Professor Viner's spine. His strictures are pertinent. The authors rushed helter-skelter for the recipe of "direct controls over inventories and selective controls over prices", and the targets for the stabilization of international trade were excessively rigid and quite impractical. After its issue we proceeded through a further period without any marked unemployment (and with a good deal of inflation) and the pattern of international trade has moved a long way towards equilibrium under the influence of natural forces—as Keynes, reverting at the end of his life to the doctrines of Adam Smith, predicted that it would.

But now that more time has passed and that the post-war forces of expansion are spent or waning in strength, and that the free enterprise countries are going through an unfortunate phase of hesitation and weak progress, thereby losing caste with the uncommitted world, would it be altogether unwise to think again in terms of targets of some kind ? We need not revert to the rigidities and impracticalities of the report, which Viner rightly condemned. But was there not something right in the actuating impulse of the authors of the report, even although we may much disagree with the conclusions to which it led them ? Expansion in accordance with growth potential, rather than full employment, should now be the criterion. Does the present situation perhaps need the fierce and scornful adjurations of Keynes rather than the sagacious sobriety of Viner ?

I was intrigued to see the re-appearance of his scholarly article on cost and supply curves (reprinted from *Zeitschrift für Nationalökonomie*, 1931), which I read at the time of its publication. Viner comments, " I do not take advantage of the opportunity to revise my 1931 article. Even the error in Chart iv is left uncorrected, so that future teachers and students may share the pleasure of many of their predecessors of pointing out that if I had known what an envelope was I would not have given my excellent draughtsman the technically impossible and economically inappropriate assignment of drawing an *AC* curve which would pass through the lowest cost points of all the *ac* curves and yet not rise above any *ac* curve at any point ". I had already drawn these curves myself (in an article which appeared in the *Economic Journal*, December, 1931), before Viner's article appeared, not in the way that Viner wished his draughtsman to draw them, but in the way that his draughtsman insisted on doing. I remember vividly—it seems like yesterday—wandering across Tom Quad in Christ Church in quest of a scientific colleague. I showed him my curves. " Look at these curves," I said, " there must be some name in mathematics for this outside curve." " Yes," he replied, " it is called an envelope." As I used this word in my *Economic Journal* article (1931) and again in the *Quarterly Journal of Economics* (1934), I like to think that my walk across the quadrangle may have been responsible for initiating in economics the correct usage, for neglecting which Viner has been so much taunted !

Viner refers in a footnote to having seen my 1930 article between 1950 and 1951, and I take it that this applies to the 1931 article also, the latter being the one referring to the " envelope ". It makes me a little wistful that Viner, despite his scholarly devotion to origins, did not see my article for twenty years after it appeared; I feel the more wistful since that article contained the first algebraic formulation, in terms of elasticity of demand, of the marginal revenue curve, which has played so prominent a part in economic treatises and textbooks ever since. The previous article (also unseen by him) contains a literary account of the marginal revenue curve and an accompanying diagram; it was written in 1928, rejected by the *Economic Journal*, later re-submitted in substantially the same form, and published there in June, 1930. It also contained a demonstration why, save in one case, the production possibility function must be concave to the origin (or why there must be diminishing returns to factors).

If we are interested in origins, it is noteworthy that there is in this collection an article (" Price Policies; the Determination of Market Prices ") which, although slight, gives, as early as 1921, a clear account by Viner of the categories of perfect competition, imperfect competition and monopoly.

ROY HARROD.

The Logic of Scientific Discovery. By KARL R. POPPER. Hutchinson & Co. (Publishers) Ltd. 1959. 480 pp. 50s.

Logik der Forschung was published in 1934, a slender, soft-covered volume, No. 9 in the Vienna Circle's series " Schriften zur Wissenschaftlichen Weltauffassung ". This " revival ", as Professor Popper calls it, comes in a much more solid and stately format and consists of a translation of the original, plus an extensive sprinkling of additional footnotes, 150 pages of new appendices, and a vigorous new Preface. This new English edition, long over due, now gives Professor Popper's great work the embodiment which it and its students deserve, though its teachings have scarcely been in need of " revival ", at least since Professor Popper has been in London to champion them. Furthermore, a second volume or " Postscript " is now promised, sub-titled " After Twenty Years "—though, according to our arithmetic, it is twenty-five already.

As well as appearing in the Vienna Circle's series, alongside the works of Schlick, Carnap, Neurath and the rest, *Logik der Forschung*, or its ideas, was debated further by Popper and others in the Circle's journal *Erkenntnis* and at their international conferences. To describe Popper as a Turgot to the Circle's Physiocrats would probably place him much closer to the Circle than he would ever have accepted. But in the much more variegated philosophical setting, especially as regards this country, of twenty-five years ago, the ground Popper had in common with the Circle, as well as with Wittgenstein and English analytical philosophers, was relatively much more important, and was then much more widely disputed by other influential philosophical schools, than it is today. Today, something of a revolution in this country having put so much—at any rate locally and temporarily—almost beyond dispute, the points of difference between Popper's views and those of his " allies "—as he happily calls them (p. 15), though not invariably treats them—stand out in much greater relief. But it is difficult not to feel some sympathy with Carnap's complaint in 1935 that Popper had rather strongly emphasised his differences with his " allies " and " de-emphasised " the comparatively large area of common ground. Furthermore, as he eloquently makes plain in his new Preface, Professor Popper shared and shares with the old Vienna Circle—unlike the English " ordinary language " philosophers—a primary enthusiasm and reverence for science and the progress of scientific knowledge (i.e., a sort of " Wissenschaftliche Weltauffassung ").

Anyhow, someone trying to apply to the methodology of economics some of Professor Popper's (and his allies') leading principles would find a much wider readiness to accept their implications and relevance than in the middle thirties. Then they would have been authoritatively denounced for " fashionably " trying to equate the methods or criteria of economics and the social sciences with those of the natural sciences, while the principle of testability or falsifiability would have been dismissed as impossible or irrelevant; and such criticism would have come not simply from an extremist fringe—as it might still today—but from leading authorities of the period.

Central to Professor Popper's entire account of scientific methodology is his proposal of *falsifiability* as a criterion or *principle of demarcation* between " science " and " non-science " (not, be it emphasised, between " sense " and " non-sense " as in Wittgenstein's earlier use of the *verification* principle). From this follows his dismissal of inductive methods and inferences, including probability logic based on them, and his concept of scientific laws—capable of " corroboration " but not verification—as *forbidding* events (somewhat resembling Mach's account of them as " Einschränkungen der Möglichkeiten "). The convention of falsifiability rather than verifiability is based on the asymmetry between the two, in that a scientific law can never be conclusively verified, however numerous the corroborating instances, whereas a single instance may be sufficient for falsification.

It is specially interesting—as Professor Popper has told us elsewhere—that he developed the falsifiability criterion between science and " non- " or " pseudo-science " in trying to assess theories of human and social behaviour (Marxian, Freudian and Adlerian). This illuminating demarcation principle—a great advance on the old verification principle of " meaning "—surely has much relevance throughout human and social studies, as does Popper's searching analysis of " conventionalist strategems " for evading falsification or falsifiability. It is, therefore, specially intriguing to detect in his latest notes (*vide* p. 206) what seem to be signs of a readiness to relax the formulation or application of his demarcation principle, though we shall have to wait for his " Postscript " to see just how far Professor Popper may be ready to go. He still sees the problem of apt or adequate " demarcation " as the key to most of the fundamental problems of the methodology or philosophy of science. Of great interest will be any further analysis of statements or doctrines from the " non- " or " pseudo-scientific " side of his demarcation line. Some of these may, apparently, now be admitted as " rationally arguable ", although not " falsifiable ". Is this a new demarcation line, and, if so, just what will be found on either side of it, and how are such doctrines to be marked off from those of " post-rationalism "—the latest exhibit in Professor Popper's private zoo of intellectual monsters (alongside such old favourites as " positivism ", " historicism ", " essentialism ", etc.) ?

Though much of it is devoted to fairly technical problems well outside our range, no reader can miss in *The Logic of Scientific Discovery* that quality of powerful, exhilarating originality which marks all Professor Popper's writings. There is much in this book that lights up, or can be directed to lighting up, many of the methodological issues that economists, or some of them, have bothered over, and perhaps still need to bother over—for example, in regard to the perennial problem of " value judgements " raised again in recent unsatisfactory controversies over monetary and " growth " policies.

T. W. HUTCHISON.

National Wages Policy in War and Peace. By B. C. ROBERTS. George Allen & Unwin Ltd. 1958. 180 pp. 15s.

Among all the economists who have written or lectured on wages policy in this country in recent years—and which among us has escaped doing so ?—there can be few who are not gratefully indebted to Mr. Roberts for his own publications in this field. It is upon his deep understanding of British labour and trade unions that we have drawn to give factual verisimilitude to our large generalisations on this vexed topic. And it is to his researches into the policies and movements in other countries that we have turned for evidence to support our own views of what should be done here, or to confound the recommendations of those whose nostrums we find distasteful.

One approaches this book, therefore, with very great respect for its author and with high expectation of adding substantially to one's stock of knowledge of happenings in the wages world. And one is disappointed. To be sure, it is a useful book, recording much that has gone on in the United States, Australia, Sweden, Holland and Western Germany as well as in this country in the last 15–20 years and in some cases (more briefly) over an even longer period. But it is not the authoritative study which Mr. Roberts's past work had given us reason to expect. Certainly, wages policy is an intractable subject and it would be difficult for anyone to write an entirely satisfactory book about it. But that is not the only source of trouble in this case. In addition to a confusing chapter arrangement—a defect which the reader can easily remedy for himself—the book has three major blemishes.

First of all, Mr. Roberts breaks most of the elementary rules of statistical presentation. In some cases the effect is merely to irritate the reader—the attachment of a term to a single column which belongs to several (Table 9), or giving the label, " increase " to a straight index number (Table 23). In others, however, it is more serious. For example, one can get the general sense of the Swedish economists' important concept of " wage drift " (Table 9) from Mr. Roberts's text, but nowhere is any precise definition provided. More generally, though sources are quoted, seldom if ever is the exact coverage of a particular series defined. In the more familiar cases one can provide one's own check. One is not surprised, for instance, to find that an American series described at one point as " average weekly earnings " (Table 5) is in fact confined to manufacturing industries (Table 7). But what is one to make of the Dutch, Swedish or Australian data ? One would feel happier with them as they stand if Mr. Roberts had himself always used his statistics with due regard to their particular coverage and limitations. Unfortunately, one cannot be too sure of this.

This is the second blemish. Quite early on (p. 33) one finds Mr. Roberts stating that the wartime government of Britain " decided to aim at stabilizing the cost of living index at 125 to 130 of the pre-war level ", and pointing to his Table 1 as demonstrating that they failed. Table 1 shows nothing of the kind, for the index of retail prices there given is the London and Cambridge index compiled after the war by Professor Allen. The policy of the time was specifically to stabilise the old Ministry of Labour index—which the Government did. If, then, one can fault Mr. Roberts in his handling of statistics with which one is familiar, how much can one rely on his handling of the unfamiliar ? Certainly, the connection between argument and the figures given is not always clear. A close reading of the Swedish chapter, for example, relating text to tables, leaves one taking quite a bit of Mr. Roberts's argument on trust.

This brings one to the third complaint, which relates to what one may perhaps best describe as Mr. Roberts's double standard of critical enquiry. As long as Mr. Roberts is tracing out the labyrinthine courses of wages negotiations in the various countries one has confidence in him as a guide. In particular, his final judgment rings true, that " whatever in theory might be said in favour of a centralized system of wage controls, it cannot be said that those examined in the previous chapters have succeeded in preventing inflation ". But Mr. Roberts is rightly aware that the problem of inflation is far from being merely a matter of wage negotiations and negotiating machinery: these are tied up with general policies of economic control. Now, the fact that Mr. Roberts sees these latter for the most part in the simple terms of controls over the supply of money may not in itself be a fault—though frequent reiteration of the doctrine throughout the book will no doubt irritate many readers who do not see the answer so simply. The complaint is that Mr. Roberts does not submit the claims made for monetary policy to the same class of empirical examination as he applies to wage controls. For wage negotiations, justification by works is the standard; for monetary policy, justification by faith is accounted enough.

At the outset of his enquiry, Mr. Roberts describes " the critical question " as " whether the Government should normally confine its policy to the exercise of influence on the level of wages by means of its monetary and fiscal policies " (p. 24). At the end, he declares that " a national wages policy is redundant if the aggregate level of demand is not permitted to exceed that level required to maintain full, but not over-full employment ". " If there is no demand inflation then cost inflation must soon peter out " (p. 164). The brief treatments of credit restriction (" the most simple way of ensuring that the level of demand does not rise too rapidly ") in the chapters on Sweden, Australia and the Netherlands are scarcely adequate support for this contention. The late reference to recent experience in the United States in the final summing up—detailed discussion of the U.S. ends with the breakdown of the Wage Stabilisation Board in 1952—is in admitted contradiction of it.

Mr. Roberts should write a second edition of this book, subjecting monetary policy to the same thorough and critical enquiry as he has applied to wages and wage negotiation.

R. C. TRESS.

The Economic Theory of Fiscal Policy. By BENT HANSEN. George Allen & Unwin Ltd. 1958. xv + 450 pp. 45s.

During the last decade several loosely drawn generalizations of neo-Keynesian fiscal theory have been subjected to rigorous, critical inquiry. Recent work on the balanced-budget multiplier is an example of such progress. A general treatise on the economic theory of fiscal policy is needed, and, when such a book is written, Bent Hansen's efforts in clarifying much of the discussion must be acknowledged. But this book is not the definitive work that the title might suggest.

The book was prepared originally as a report to the Swedish government. In several particular instances, Hansen's meticulous analysis produces conclusions that are original and correct. In a broader sense, Hansen's emphasis on the necessity of specifying carefully the characteristics of analytical models represents doctrinal progress. He classifies his variables with care, and he demonstrates that the theory of fiscal policy is limited to predictions of the effects of changes in State *parameters of action* such as tax rates, subsidy rates, wages of civil servants, rates of purchase of public goods, etc. Budget surpluses or deficits are not parameters of action, and the effects of policy cannot be discussed adequately in these terms. This approach lends itself to a more careful consideration of alternative means of accomplishing the objectives or ends of policy, and the bulk of the work is devoted to showing how full employment and a stable price level may be achieved by various combinations of fiscal policy means.

One of the weaknesses of the book is the mirror image of its primary virtue. While it is useful to bear in mind that the effects of a deficit, for example, depend on how the deficit is created, it is misleading to imply that, unless these means are fully specified, one prediction is as good as any other. Additional rigour is costly, and it is obtained only with a sacrifice of generality. The model builder must use his own best judgment, a quality that is not infrequently absent from Hansen's work. He seems equally interested in demonstrating how, under certain unique conditions, a budget deficit can exert a contractionary influence, and in showing how, under most sets of circumstances, the deficit will exert an expansionary influence. For these reasons, the book is marred by tedious side excursions into possible but improbable reaction sequences.

In certain other respects Hansen's basic model severely restricts the conclusions reached. I shall select here only one limited example to illustrate this point. Assume an initial equilibrium situation with full employment and price-level stability. Impose upon this situation a general increase in resource productivity. The problem is to neutralize this increase in productivity through fiscal policy means. Hansen's solution is an increase in the rate of indirect taxation. Apparently he reaches this solution in the following manner: If no action is taken, the increase in resource productivity will reduce prices. Indirect taxation will increase prices. The increase in the tax rate will generate a budgetary surplus; this will reduce the net liquidity in the economy (money plus government debt instruments). But the monetary authorities may keep the rate of interest (the single important monetary policy parameter of action) unchanged by exchanging money for debt instruments.

In an alternative, and to this reviewer a more acceptable, model the supply of money is treated as an exogenous variable. An increase in the rate of indirect taxation cannot lead directly to an increase in the price level without some accompanying change in this monetary policy parameter. In such a model, a neutralizing fiscal policy would suggest a deficit, not a surplus, and a monetary policy shift that deliberately alters the money stock to finance the deficit.

The two models yield opposing conclusions; Hansen's suggests surplus financing and the alternative deficit financing. The differences are more apparent than real. The increase in the stock of money in Hansen's model is obscured as an induced shift in the value of an endogenous variable. If the monetary authority's purchase of government securities is added to government "expenditures" more normally defined, the Hansen model can be said to imply deficit financing as a solution to the shift in resource productivity. By such tortuous processes, Hansen's models can be made to conform with more orthodox models. Few will carry out such modifications, however, and because of this, Hansen's theory is likely to be misleading as a guide to policy.

Does Hansen's analysis offer any guidance on the vital question concerning the relationship among wage increases, price-level stability, and full employment? His solution is that of matching wage increases in excess of productivity increases by reductions in indirect taxes (increases in subsidies) accompanied by increases in income tax rates. By this combination of means, he states that fiscal policy can preserve both price-level stability and full employment. But for what? Hansen does not say, nor is he interested. The fact that the combination of policy means suggested will produce results substantially equivalent to open inflation does not seem to have occurred to him.

To a reviewer familiar with the American political structure, the idea of a single government decision-maker deliberately manipulating the various State action parameters to maintain both full employment and price-level stability seems far removed from reality. In the institutional environment of modern Sweden, the analysis is surely more applicable. A more careful consideration of the political and institutional framework of fiscal policy would have limited the application and increased the value of the analysis.

JAMES M. BUCHANAN.

The British Paper Industry, 1495–1860: A Study in Industrial Growth. By D. C. COLEMAN. Clarendon Press, Oxford. 1958. xvi + 367 pp. 55s.

This book is a valuable addition to the growing number of case-histories of industrial evolution. Further, it may well be considered a pioneering study in its approach, for Dr. Coleman has found sufficient quantitative evidence for the period before 1850 to analyse the process of early growth in the paper industry and its changing structure much more rigorously than has proved possible with industries which did not suffer taxation at the point of production. Although the source of such statistics marks them as suspect in some respects (they involved the extraction of money from those who supplied the figures, or brought the hope of export bounties), with due regard for their frailties, the historian of an excised product is in a fortunate position. This book may be seen as one of the first harvests of the mine of information in the archives of the Customs and Excise, which evidence is buttressed by an equally diligent and critical use of the Parliamentary Papers for the period after 1800. Many more economic historians will be following Dr. Coleman to the cellars of King's Beam House.

The paper industry had certain special features from the beginning. It was a mill industry, many early sites being adapted from corn-milling; hence fixed costs were never negligible and would-be paper-makers had always to overcome a barrier of pre-mobilised capital to enter the industry. Its labour force was always modest (only 7,300 were employed in paper mills in Great Britain in 1841 after three centuries of development); moreover, semi-skilled workers, such as characterised the cloth industry, were never a significant proportion of the labour force. On the other hand, the smaller groups of highly-skilled operatives developed effective combinations, and preserved their status, until their position was undermined by the Fourdrinier machine in the early nineteenth century. As so often, mechanisation was welcomed by employers anxious to overcome the intransigence, as well as to avoid the higher costs, of highly-skilled workers. The chapters on labour and combinations in the industry are linked very well to the analysis of changes in production.

A further merit of the book is in the treatment of demand, which has been often taken for granted in industrial history, or at least neglected for a detailed narrative of technical changes. Dr. Coleman establishes that export markets were never of much significance for the paper-makers; rather, their problem for two centuries after the establishment of the industry was to resist the import of high-quality paper from France and Holland, in which stage of its growth customs-protection was important. The paper industry was thus one of several skills in Britain which were with difficulty transplanted from the Continent in the sixteenth century, and the development of which was a continuous battle to produce higher-quality products which could compete effectively with those from the older areas of production. The market for coarse wrapping papers was captured in the late seventeenth century, but not for a further hundred years could British paper-makers call the national market in high-grade writing papers their own.

Production thus expanded originally by the replacement of imports as much as by the expansion of the home market itself. This experienced a slow development from increased uses of paper for wrapping, printing and writing; but only from 1750 did demand rise rapidly. Total population then began its unprecedented and sustained expansion, while, equally important, the structure of society was changing in ways which led to a more intensified need for paper. The trend to mass literacy was leading, via the mechanisation of printing, to mass communication; the business world needed ever more paper upon which to conduct its affairs; while a succession of new uses, such as wall-paper and tarred paper, appeared, which continued to augment the traditional roles of this versatile material. Per capita consumption rose from 1·5 lb. per annum in 1715 to 2·5 lb. in 1800 to 8 lb. in 1860. The fact that demand has continued to widen as well as deepen to the present day (annual consumption is now about 130 lb. per head) has enabled the paper industry to defy the usual " logistic curve " of an industry's production and maintain its rate of expansion.

Plentiful statistics have allowed an equally detailed treatment of the effects of industrialisation between 1800 and 1860. There was the usual portentous increase in production (from under 11,000 tons to above 100,000 tons in the U.K.). Greater localisation of production developed in the South, as mills shrank back upon the most favoured sites in Kent and Buckinghamshire, and new mills sprang up in the Midlands and the North. The total number of mills fell slowly, from 434 to 377, the average size of plant rose steadily and there was an equivalent increase in the dominance of the large firm (by 1860, 7 per cent. of firms possessed 33 per cent. of output). In the period of Dr. Coleman's investigation, to which unity is given by the use of rags as the basic raw material, such consequences of mechanisation upon the structure of production were mitigated by the continuing increase in demand. Indeed, so triumphantly were the problems of production overcome and so continuously resilient was the market, that only wood-pulp could avert a crisis in the supply of raw materials, despite the new use of bleaching agents, straw and waste from textile mills to stretch out the supply of rags. A leading paper-maker declared in 1861: " I think there is no probability of any substitute for rags being discovered ". It is refreshing to find the generalisation confirmed that such affirmations of impending doom appear just before the problem is overcome.

Throughout the study the text is terse, to the point and written with marked economy. Its scope is determined by the nature of the primary source material, shrewdly deployed to answer the main problems about the process of growth in the industry, and by the nature of the enquiry itself, which tends always to view the industry in the whole, as a changing aggregate. One might regret, however, that Dr. Coleman did not allow himself to linger in a little more detail upon the paper-makers themselves—or even to expand his description of developing technology after the first introduction of the Fourdrinier machine.

PETER MATHIAS.

Smiths the Bankers, 1658–1958. By J. A. S. L. LEIGHTON-BOYCE. National Provincial Bank Ltd. 1958. xiv + 337 pp.

Histories of private banks are all too often little more than biographies of their reigning dynasties. This book is a welcome exception.

The house of Smith founded banks successively in Nottingham, London, Lincoln, Hull and Derby, and flourished increasingly down to about the last quarter of the nineteenth century. Mr. Leighton-Boyce believes that the Nottingham bank, long regarded as the oldest provincial bank, was founded some 25 years earlier than has been commonly assumed, i.e., about 1658. Thomas Smith I then began a connection with Edward Blackwell, in London, which left him a creditor in the latter's bankruptcy. Thomas Smith himself was appointed, a little later, a sub-commissioner of excise for the County of Nottingham, thus beginning a link with the remittance of tax receipts to London which became increasingly important as time went on. Mr. Leighton-Boyce reminds us again that the right to transmit such funds was of great importance to country bankers, not least because of the opportunities for profitable use of them in the interval between their collection and their payment into the Exchequer.

The second stage in the history of Smiths opened with the partnership between Abel Smith II and John Payne, out of which came the London bank of Smith, Payne (1758). This acted from the beginning as investment agent for the Nottingham bank, and also in due course for Smiths of Lincoln (founded 1775), Hull (1784) and Derby (1806). During the dominance of Robert Smith I (Lord Carrington), the surplus funds of the whole group were gradually concentrated in London. Smith, Payne also acted as London correspondent for a number of other country banks.

The homogeneity of the group survived the various banking crises and developments down to about 1890, but seems then to have been impaired by a clash between the go-ahead provincial banks, headed by Lincoln, and the more conservative, not to say reactionary, London bank. The latter frustrated for some years attempts to weld the group into a single entity capable of meeting the joint-stock banks on their own ground. This was at length achieved, in 1902, by a fusion with the Union Bank of London Ltd. The united bank maintained its independence until 1918, when it joined forces with the National Provincial Bank of England Ltd. It is the National Provincial Bank which has sponsored the present history—and it is to be congratulated on its enterprise.

The book necessarily follows the fortunes of the Smith family in some detail, and their complicated genealogy is something of a tax on the reader. But at least as much attention is paid to the principles and practices of the banks which they founded. Particular interest attaches to Mr. Leighton-Boyce's studies of the liquidity and lending policies of the five banks, which he shews differed between themselves and over time. In this and other respects new light is thrown on some well known facets of banking history, and we are able to watch economic events, as it were, from a seat in the bank parlour. The economic historian, as well as the banking specialist, will find a study of the book well worth while.

J. K. HORSEFIELD.

A Survey of Social Conditions in England and Wales. By A. M. CARR-SAUNDERS, D. CARADOG JONES and C. A. MOSER. The Clarendon Press, Oxford. 1958. xxi + 302 pp. 25s.

Carr-Saunders and Caradog Jones' *Social Structure of England and Wales*, published in 1927, and its later edition in 1937, have proved such useful books of reference that an up-to-date edition, under a slightly revised title, is most welcome. Though " Structure " now becomes " Conditions " owing to the capture of " structure " by sociologists, the contents are not radically altered, and " conditions ", like " structure ", are still surveyed " as illustrated by statistics ". Official and other statistics have been greatly extended and improved, and we are now treated to 148 instead of the 82 statistical tables of the original edition.

The chief additions are in vital statistics, with three chapters instead of one; and special chapters on Nutrition, Health, Use of Leisure Time and Religion. Provision of an index is also a great advantage over the original edition. One or two topics have, however, been dropped. Gone is the original chapter on Poverty, which contained a valuable review of the Booth-Bowley-Rowntree surveys, and which, with Rowntree and Lavers' recent *Poverty and the Welfare State*, would surely have formed quite an epic of our virtual abolition of primary poverty. Gone also is the chapter on inborn qualities, together with references to Sir Cyril Burt's work. Possibly what is inborn and what is " acquired " are now considered too uncertain, or the quality of individuals is not thought a social " condition ". But it seems difficult to appreciate educational conditions without detailed reference to the proportion of children that are educable up to various levels, or are mentally deficient; deficiency is dealt with in the chapter on Health. It seems, incidentally, that our mental hospital, is still well ahead of our university, population.

Among the material so usefully added we may pick out social mobility, perhaps, and also certain trends made manifest by the lapse of time since statistics were first collected.

From the four enquiries into mobility that are quoted, it appears that—using as tests type of school and university—school teachers and industrial managers were the most democratically recruited, then civil servants, but, last of all, bishops of the Church of England. Thirty-eight out of the forty-three bishops had been to two particular universities which were still fairly exclusive when most of them were undergraduates. Among the more interesting trends disclosed are: the rapid growth of the New Towns (pp. 55–6); the apparent reversal of the " law of tertiary industry " that the march of civilization involves more services—the six services industry groups formed (Table 8·2) 49·3 per cent. of the occupied population in 1931, only 41·8 per cent. in 1951; the reduction in the class differential between death rates (p. 13), but not apparently between birth rates (p. 25); and the great inequality that still persists in the distribution of private property, though inequality has diminished considerably in *income* distribution.

The extension of official and other statistics makes it not less but more important to have summaries such as this book provides, to have a reasoned argument connecting up such conditions as are summarily measurable, and to have the pitfalls likely to beset attempts at further comparison and generalization pointed out. More students should, as a result, be stimulated to embark upon empirical research and yet be wisely guided.

P. SARGANT FLORENCE.

Productivity and Economic Incentives. By J. P. DAVISON, P. SARGANT FLORENCE, BARBARA GRAY and N. S. ROSS. George Allen and Unwin Ltd. 1958. 306 pp. 35s.

Productivity in a Printing Firm. By F. A. WELLS. Gerald Duckworth & Co. Ltd. 1958. 148 pp. 21s.

Professor Sargant Florence led his team back to the study of the incentive effect simply of the opportunity to earn more by doing more: " the element of the Economic Man among industrial workers requires rediscovering as a complement to the Sociological Man who looms so large in the literature of today ". He points out here that there is a great lack of inquiries isolating and measuring the effects of wage incentives, yet there is reason to believe that in their absence output runs lower than it need, and applying them is commonly expected to raise it by as much as a third: can we say how much of this is due to them themselves, and how much to the other changes that often accompany their introduction? To this end he and his team succeeded —a notable achievement—in finding some fifteen cases in which not only were records available of output and earnings before and after the introduction of payment by results, but the attendant changes in methods were small enough to make it probable that most of the change in output was due to the wage incentive. Messrs. Davison and Ross report these cases. In one, output per man-hour rose little; in nine, it rose by between 35 and 100 per cent.; in the remaining five, it more than doubled. These findings are powerful arguments.

But the same authors also have a chapter on the worker's attitudes and reactions, which shows that his suspicions of change are so deepgoing that the effect of incentives, perhaps even the possibility of introducing them at all, depends on his relationship with management. " Workpeople are aware of the carrot and the stick argument and they are inclined to believe that when an employer offers more money he is using the carrot because he cannot use the stick. They suspect that offers of higher pay with conditions attached mean that the employer has discovered a way of taking advantage of his workers." The summary of trade union views (pp. 114–117) and the conclusions (pp. 128–133) are specially to be noted as a highly instructive account of the complex interplay between the wage incentive and other motives. The incentive effect proves hard to separate after all.

This finding is reinforced by the accounts that follow of how incentives have been applied in some particular firms: Barbara Gray tells of laundries and co-ops, Norman Ross of a food factory. We see how in one laundry incentives worked well as part of the general reorganisation put through by a keen young manager, but in the food factory they were hamstrung and halted by the workpeople's distrust of the manager. This last is a remarkable though perhaps not unusual story—a head office and a trade union both in favour of extending incentives; a local manager who was felt to resent the activity of the union, was apt to make changes without consultation or warning, and before long was believed not to want the new methods to succeed; workpeople who got on well with the consultants, but brought their activities to a stop when they recommended that the work done at one point by two men and a girl should be done henceforward by one man alone. Usually it is only enlightened management that gives access to the observer, but here Norman Ross has been able to record with a telling fullness of detail what goes on in a firm where the touch of management is maladroit: we watch the breakdown of an incentive scheme in a setting of mistrust which the scheme itself deepened.

The main lesson of Mr. Wells' lucid and thoughtful study is again the interdependence of the different factors that govern productivity; he, too, brings out the central role of management, and shows how " the indirect effect of the incentive schemes on management was far more important than their direct effect on the workers. It might be suggested that the improvements in organisation could have been made without any changes in the wage system and it is at least arguable that the gain to productivity would have been as great ". There is in fact an instance here of work study being applied with the generally willing co-operation of the workers although it was accompanied by no wage incentive; and Mr. Wells touches on other ways in which the spirit and method of management can raise productivity. Management's own basic incentive, he believes, is professional pride. His work does at the same time provide a carefully documented study of the application of wage incentives: again the effect is remarkable—in a good week the best workers were able to raise their output by more than two-thirds. But by no means all weeks were good: one of the disadvantages of incentives, in the calculus of welfare that includes the worker's feelings as well as the consumer's, is their liability to engender such wide fluctuations of earnings as stand recorded here.

Mr. Wells also makes an instructive contribution to the study of costing and of pricing decisions, and gives a significant account of a major investment decision. He shows us how these things present themselves in the changing story of a firm, which he tells so faithfully as to admit us to its own mood and conclave.

E. H. PHELPS BROWN.

The Struggle for a Higher Standard of Living: The Problem of the Underdeveloped Countries. By W. BRAND. The Free Press, Glencoe, Illinois. W. van Hoeve Ltd., The Hague. 1958. ix + 438 pp.

This is in essence a translation of a Dutch study published in 1954 in the Netherlands. The excellent translation into English is to be welcomed. The book gives a comprehensive review of many economic and other aspects of the problems of development and economic growth in the economically backward countries. It incorporates much experience which Professor Brand gathered as a member of the Secretariat of the United Nations from 1946–1957. The book will be very useful to students and teachers but it is much more than a text book. It contains an excellent review of, and valuable judgments on, current economic theories and national and international policies. It follows a systematic and useful plan of exposition. As a result, the book is not diffuse although it covers an exceptionally large canvas. The author endeavours to present each of the problems examined by him by contrasting the main economic theories bearing upon them, or the views currently held or propounded concerning them, with relative statistical data or other material which throws light on the institutional factors involved. In many cases, these institutional factors are found to vitiate much theorising or wishful thinking. The systematic examination of the institutional background of economic policy and political aspirations makes it possible for the reader to obtain a vivid appreciation of the difficulties involved in the practical problems of development. These problems are illustrated by examples from a wide range of countries in Africa, Latin America and the Far East.

Professor Brand brings out clearly the nature of the challenge to modern thought and action involved in the problems which face richer nations in their attempts to raise the standards of living of the backward ones. The book falls into three main parts. The first deals with the Process of Economic Development. It examines the quantitative analysis of the inequality of international income distribution; it analyses the efficiency of use of land, labour and capital, and the obstacles thereto, and it discusses the significance, nature and methods of leadership and organisation as supplied from within and from outside the countries concerned, by individuals or by governments. The second part discusses Domestic Financial Resources in relation to the pattern of consumption: private investment; foreign trade; the Government sector; the role of the State, and the problems of inflation.

The third part deals with International Financial Resources and includes an examination of international capital movements, and of problems of private and public foreign investment. There is also a survey of post-war development of new public investment agencies. The book ends with the inclusion of a valuable bibliography as well as notes and an index.

What distinguishes this book is its wealth of illustrative material, drawn from a vast range of international, official, and other documents; its humanitarian approach, its rigorous economic thinking combined with insistence on the need for the economist to understand the limits of his subject in relation to the political and sociological factors beyond his purview. Last but not least, it should be mentioned that it has an important message for policy makers. It insists that the problems of raising the standard of living must not be confused with the problems of power politics. Power politics after all, are frequently of considerable benefit to the few, but in the last resort they have to be paid for by the many. Those who read this book will not only obtain much valuable advice as to what could and should be done, but much subtle wisdom concerning how not to do it.

S. HERBERT FRANKEL.

Introduction to Operations Research. By C. WEST CHURCHMAN, RUSSELL L. ACKOFF and E. LEONARD ARNOFF. John Wiley & Sons Inc., New York. Chapman & Hall Ltd. 1957. x + 645 pp. 96s.

The literature on operations research is growing rapidly so that whereas only a few years ago there was little to recommend today there are a number of works available of a reasonably high quality. The book under review is among the latter and will be of value not only to the intending practitioner but also to the general economist who is interested in understanding the practical applications of mathematical techniques to the operating problems of modern business. Indeed, a consideration of the enormous range of problems which are soluble by the straightforward application of the new analytical methods may lead the academic economist to a reformulation of the theory of the firm.

Introduction to Operations Research may be considered to be divided into two basic sections, one devoted to the general procedure of operations research, the other to the techniques and the solution of specific problems. The first of these endeavours to describe the sort of circumstances in which operations research is likely to be carried out (especially when it is being applied for the first time), the difficulties involved in formulating problems to be solved, and the type of model (quantitative and stochastic) which is usually constructed. In the second section are to be found a large number of actual models: of stock control, linear programming, queueing, capital replacement scheduling, and elementary game theory. In addition, some attention is paid to the questions of testing models against subsequent operations, putting improved methods into practice, and setting up an operations research team.

The approach employed by the authors may best be understood by means of an example, such as their treatment of stock control. They start by considering the case of a single commodity used at a constant known rate where the problem is to determine the batch size or purchasing quantity which minimizes set-up (or ordering) and holding costs. They then complicate matters by allowing shortages (not costless) to occur, varying rates of use, discontinuities, variable manufacturing or purchasing costs, restrictions on fixed assets and warehouse space, and multiple products. Each of the complications is discussed analytically and is then illustrated by arithmetical examples. The treatment of the other classes of model is carried out in a similar fashion.

The strength of the book lies in its coverage of a very large number of cases. Its weakness lies in its failure to provide an overall conceptual framework which would give significance to the individual pieces of analysis. Not enough attention is paid to the micro economic concepts developed for other purposes by economists. A major problem of present-day research is to transform the multiple objectives (or multi-variate preference systems) of firms into simple criteria which may be used by operations researchers to provide solutions to their particular models. In addition, since most operations research today is concerned with partial models of the firm, policy recommendations following from it may be misleading if inter relationships with other parts of the organisation are ignored.

One of the main troubles with operations research teams in the United Kingdom is their frequent ignorance of economics, and their failure to employ economists. It is to be hoped that books of the kind under review will eventually rectify this situation both on the demand side, by pointing out the need for economists in the field, and on the supply side, by causing academic institutions to interest economists in this subject and to prepare them for practical applications of their theories and concepts.

M. H. PESTON.

The Theory of Committees and Elections. By DUNCAN BLACK. Cambridge University Press. 1958. xiii + 242 pp. 30s.

This book is concerned with a problem which is more general, and more important, than its title suggests: if a number n of alternatives is arranged in order of preference by each of m individuals, under what circumstances will one of the alternatives be a clear winner, and if there is no clear winner, how should the winner be selected?

Part I of the book considers various methods of selection and their consequences, mainly in an abstract, though mathematically simple, way. A good deal of the discussion is confined to the situation where the alternatives can be arranged so that the order of preference of each individual can be expressed by a curve with no more than one peak; it seems reasonable to suppose, with the author, that if the alternatives are in fact different values of a single measurable variable (price, size, etc.), this situation will frequently be realised in practice. For this single-peaked case, a number of desirable properties (e.g., the existence of a simple majority for one alternative, the transitivity of the group's judgments) are proved under simple-majority voting rules.

The really interesting cases in practice, however, are those (referred to here as "cyclical-majority" cases) where the group judgment is not transitive, i.e., *A* is preferred to *B*, *B* to *C* and *C* to *A*. Here the author devotes himself largely to discussion of the 18th century work of Borda and Condorcet and the 19th century work of Dodgson, and the whole of Part II of the book is a historical account of their work, which seems unnecessarily full in the case of Dodgson. (Why are economists so concerned about "Alice" and its author?) Most of the content of the book is based on a series of papers published by the author in 1948–9. He deliberately excludes reference to Arrow's *Social Choice and Individual Values* (1951), but seems also to be entirely unaware of the extensive work of psychologists and statisticians on ranking theory since Spearman and Karl Pearson in the first decade of this century. For example, what is here called "Borda's criterion", that the alternative should be selected which receives the best average rank, has been studied as the "ranking concordance" problem since M. G. Kendall reinvigorated ranking theory about 20 years ago. J. Durbin has extended one of Kendall's results to show that the ranking arrived at by arranging the alternatives in order of average ranks has an optimum property: among all possible rankings it leads to the violation of fewer original preferences (properly weighted) than does any other. Similarly, the problem of intransitivity has been studied as the "paired comparison" problem in connection with the construction of a ranking from a set of intransitive individual judgments between pairs of alternatives. A full account of this and other work is contained in Kendall's *Rank Correlation Methods*, but there have been some new results even since its second edition in 1955.

On the whole, then, this book is likely to be of most interest to the historically-minded. It is written pleasantly, though a trifle heavy-handedly (it is not really necessary to prove that at most one alternative can be preferred to all others), and contains one *obiter dictum*—"the connexion between mathematical symmetry and what is ethically right may be closer than has been recognised"—which will horrify thoughtful mathematicians.

ALAN STUART.

Economic Models: *An Exposition*. By E. F. BEACH. John Wiley & Sons Inc., New York. Chapman & Hall Ltd. 1957. xi + 227 pp. 60s.

This is a textbook divided into two parts: "Mathematical Models" and "Econometric Models". There can be no doubt that Professor Beach has a deep knowledge of his subject. There are no obvious mistakes and the combination of mathematical economics with econometrics in a single textbook is to be welcomed. Yet one must hesitate in recommending this book to the student. Its price is rather stiff and the marginal rate of substitution seems to be distinctly in favour of Allen or Klein.

The trouble with this book is its lack of consistency in the knowledge it assumes the reader to have. Thus, for instance, there is quite a lengthy discussion of the most elementary theory of equations and of the calculus, but none at all of complex numbers which are extensively employed in the dynamic models. The reader is expected to know the meaning of a trigonometric function and have a nodding acquaintance with integration but to be ignorant of the distinction between dependent and independent variables or of the equation of a straight line. As the book proceeds, more and more knowledge is assumed, and the poor student who was actually learning something new at the beginning will surely give up.

There remains the student with the requisite mathematical equipment who will certainly skip the first three or four chapters. He will then come to two chapters entitled: Continuous Dynamic Models and Sequence Models. These are essentially summary chapters which quite tersely expound a number of well known models and lay down a routine for solving them. There is too little economics for this student and he will have to supplement his reading either from Baumol or the originals. He then comes to the statistical part which starts with The Elements of Statistical Theory, goes on to simple and multivariate regression and concludes with a chapter on multiple relations (simultaneous equation methods, identification, etc.). Here he will certainly learn something but will have to take long statements and formulae on trust. If he is not a statistics specialist and is just seeking general information, this will do admirably. If he really wants to learn this sort of stuff, he will once again have to go elsewhere.

Professor Beach clearly intended to write a short book, on the elements of model building and their statistical testing. The intention is very laudable. That he has failed to bring it off is not due to any lack of ability on his part, but to the simple fact that it cannot be done. A short book on models without statistics—certainly. A short book on simple econometrics without all the bits and pieces of mathematical economics—assuredly. But not both at once.

F. H. HAHN.

Income and Wealth. Series VI. Edited by MILTON GILBERT and RICHARD STONE. Bowes & Bowes, Cambridge. 1957. xiv + 306 pp. 42s.

This volume contains ten of the twenty-five papers presented at the Fourth Conference of the International Association for Research in Income and Wealth, held in Denmark in 1955. Three papers—by Christ, Lips and Schouten and P. J. Bjerve—relate to model-building, and provide a link with the more general treatment which this topic received in the Series IV Volume. The emphasis here, however, is rather on results and their critical appraisal. The other seven papers break what was then new ground in the proceedings of the Association in discussing various aspects of the analysis of income distribution.

In common with several of its predecessors, this volume as a whole achieves its purpose in being both informative about and representative of work being done in Western Europe, the Americas and Australia; and the editors are to be congratulated on a difficult task well performed.

The first paper, by Carl F. Christ, is an admirably comprehensive review of the performance of nine Tinbergen-type econometric models applied to American inter-war and post-war data. For the benefit of those who are not yet quite fully armed for "assault courses" in second-order difference equations, there is a most helpful introductory parade of econometric equipment. The main interest of this concise contribution lies in the appraisal of the predictive capacities of the nine dynamic models of the United States economy, a test from which the Klein-Goldberger model emerges with the least penalty-points.

One of the prime movers in the application of these techniques in forecasting the principal economic flows has been the Central Planning Bureau of the Netherlands. It is of special interest, therefore, to find a paper contributed by J. Lips and D. B. J. Schouten in which the system of equations used by the C.P.B. is put through a rigorous test of reliability. Year to year changes in price and volume levels of major variables from 1949 to 1954 are computed and compared with observed data. The writers adopt a margin equal to 20 per cent. of actual changes as an admissible degree of error, within the limits of which a significant proportion of their results are shown to fall. The formidable difficulties that confront the extension of short-term forecasting techniques to the prediction of bank liquidity are well illustrated in the third paper, by Petter Bjerve, in which he makes skilful use of interdependent monetary relationships to predict changes in the claims on the Norwegian Central Bank.

Of the other seven papers, those by Kjeld Bjerke on changes in income distribution in Denmark from 1939 to 1952, and by Simon Goldberg and Jenny Podoluk on Canadian statistics of income distribution—sources and methods—provide a plethora of detailed information. The paper by Dorothy Cole and J. E. G. Utting is, not unexpectedly, confined to an analysis of some of the data obtained from their Cambridgeshire Survey. A minor criticism here is that one has to wait until Table XXIII (in the Appendix) to discover the years to which their figures relate—*eheu fugaces anni labuntur, postume, postume*. A variety of methodological points is discussed by H. P. Brown in the light of Australian data for 1942–43. Odd Aukrust's paper analyses the behaviour of national income shares in Norway over the period 1930 to 1955, while those by Dorothy Brady and by Ragnar Bentzel are less concerned with the details of American and Swedish material than with the more provocative general problems of interpretation of various measures of income distribution.

IAN G. STEWART.

The Economy of Pakistan. By J. RUSSELL ANDRUS and AZIZALI F. MOHAMMED. Oxford University Press. 1958. 512 pp. 45s.

For the first few years after Partition it was difficult to secure much data, official or otherwise, even recounting—much less analysing—economic changes in Pakistan. Great efforts have recently been made to provide more adequate official statistics, whilst planning has involved the publication of many reports and other material, although there is still a great lack of detailed studies of particular problems and aspects of the economy.

The Economy of Pakistan is the first attempt to provide a general picture and analysis of the main economic problems and trends since 1947. This work is, therefore, to be most warmly welcomed, and the task has been efficiently tackled. The text is not only informative but also notably objective on controversial questions, and is illustrated throughout by tables compiled from official sources.

The authors of a pioneer work of this kind, covering so much ground, and inadequately backed by reliable, detailed data, could not have been expected to say the last word even from the point of view of presenting an accurate picture of the economy. Two defects may be specifically mentioned. The first is that too little warning has been given of the unreliability and need for careful interpretation of the official statistics, which, without further explanation, may prove misleading. One example is Table IV (p. 23) which gives the "Occupational Distribution of the Labour Force" for 1931 and 1951. These figures show increased dependence upon agriculture, whereas since 1948 one of the most striking developments in Pakistan has been the remarkable expansion of consumer goods industries and a substantial increase in certain other non-agricultural pursuits. This apparent discrepancy can be explained by a number of factors, including the need to allow adequately for the disastrous effects of Partition, which denuded Pakistan (by Hindu migration) of a large proportion of her non-agricultural population, and East Bengal of its former market, Calcutta. This has meant that a fresh start had to be made, after Partition, to construct a more balanced and viable economy.

The second defect is that although facts and figures are presented for West and East Pakistan separately, insufficient emphasis has been placed on the problem involved by the serious conflict of interests between the two wings of the country.

An interesting and extremely important chapter deals with land tenure, which brings out clearly the deficiencies of both post-Partition land legislation and its implementation, and reveals the intricacies of the regional problems involved. In the space given to this subject, however, it has not always proved possible to clarify the exact situation in each region.

All in all the work is to be highly recommended as an excellent introduction to a survey of a fascinating instance of the problems of an "underdeveloped" country.

VERA ANSTEY.

The Board of Trade and the Free-Trade Movement, 1830–42. By LUCY BROWN. Clarendon Press, Oxford. 1958. vii+245 pp. 30s.

This is a useful addition to our knowledge both of the making of economic policy and of the position and working of the higher civil service in Early Victorian England. Dr. Brown shows what great latitude was allowed to the senior officials of the Board of Trade not only to formulate the general lines of commercial policy but also to disseminate their ideas on the subject, both at home and abroad, with little guidance or check from the government. But she also analyses the political environment to indicate why, in spite of such unchallenged freedom, little was done by the Whig governments to implement the free-trade programmes that were being hatched in their midst. As a final touch of ironic comedy she demonstrates that when Peel came to power in 1841 his government distrusted the Board of Trade officials whom it inherited from the Whigs and gave them far less scope—but immediately imbibed their ideas, partly from publications which those same officials had anonymously inspired, and applied more of them than the Whigs had ever done.

Dr. Brown also gives new information about some of the internal workings of the Board of Trade, of which the account of how Porter set about the running of the Statistical Department is particularly valuable. Her presentation of what was thought and done in relation to commercial policy from 1830 to 1842 is admirable. What she says about the truth and relevance of the ideas and the permanent value of the actions is sometimes more questionable. She is so careful not to be caught in any of the illogicalities which some of her officials uttered and not to overlook any of the gaps in the empirical basis of their arguments that she leaves the impression of having, unintentionally, under-rated their positive virtues and achievements. Anyone unacquainted with *The Progress of the Nation* would fail to guess from her discussion of Porter what a wide variety of interests and information are represented in that invaluable work, though he would be well guarded against its shortcomings. And a less perfunctory consideration of the course of trade might suggest that there was more justification for the Board's emphasis on the effects of foreign tariffs than Dr. Brown allows. Nevertheless, her book is a valuable reinforcement to the evidence which compels increasing attention to the role of the civil service in public policy from 1830 onwards.

W. ASHWORTH.

The Australian Wool Market, 1840–1900. By ALAN BARNARD. Melbourne University Press. Cambridge University Press. 1958. viii + 238 pp. 35s.

Economic historians dealing with the second half of the nineteenth century have paid more attention to industrial developments in the manufacturing countries than to the widening search for the raw materials needed to keep the factories supplied. Dr. Barnard has made a well-documented study—using British and Australian business records, together with Australian newspapers, periodicals and official publications—of one important item of industrial consumption.

His main theme is the shift of centrality in the Australian wool trade from London to Australia after 1870, occasioned by falling prices, made possible by improvements in international communications and exports of British capital, and strengthened by direct orders from manufacturers on the continent of Europe and in the United States. The book, in fact, provides an interesting commentary upon the " Great Depression ". It also brings out the extreme complexity of raw material supply. Although the classification of types of wool had not gone very far before 1900—there were nothing like the 1,536 categories of the Second World War—even the limited range of the nineteenth century poses formidable problems for the historian of the trade who, in any case, has his hands full with changes in organisation and with the sources of finance which largely determined these changes. This complexity is particularly emphasised in the final chapter of the book where prices are considered. Indeed, the many-sidedness of the subject has obliged Dr. Barnard to produce what is, in fact, a series of studies in the marketing of Australian wool rather than a connected story of the growth of the trade. It is a pity that he could not have made more of the impact of business men (and, in particular, of T. S. Mort, whose private papers he was only introduced to after the book had gone to press), for a stronger biographical approach might have helped to pull together the various parts of the story. No doubt the history of Goldsbrough, Mort and Co. Ltd., which Dr. Barnard has in prospect, will take this useful series of studies a stage further.

T. C. BARKER.

Mengenplanung und Preisplanung in der Sowjetunion. By HANS HIRSCH. Kyklos-Verlag, Basel. 1957. x + 195 pp. Dm. 21·50.

This book deals with the techniques of economic planning and control used in the U.S.S.R., with special reference to the theoretical and practical problems arising from the simultaneous use of detailed physical planning and a system of prices.

There are four chapters. The first describes the machinery of quantitative planning, including the distribution of authority among the various bodies, methods of building up overall plans and techniques of control. The second deals with the place of financial planning and market prices, and discusses why these methods are required when central authorities lay down detailed physical plans. The reasons include the need to lump different goods together in overall planning, the possibility of providing special incentives, and the use of financial criteria in supervising a large number of enterprises. Now although financial incentives are used, prices in the Russian system are not determined in a way which would be appropriate if profit were the criterion for the allocation of resources. The resulting conflicts between physical plans and the incentives created by the pricing system are described in this and later chapters. The third chapter is concerned with the relation between physical and financial planning. There is a clear discussion of the relationship between three connected questions, namely (1) whether the assortment of goods is determined by consumers or by government, (2) whether physical planning is used or a pricing system (or both) and (3) whether or not administration is centralised. The author insists rather heavily on the compatibility, in principle, of a pricing system with the achievement of a given physical plan. (Incidentally the reservation about decreasing cost activities is missing here.) There is an interesting discussion of the division of authority over investment and prices among the administrative levels. The fourth chapter deals with the structure of prices, describing the bewildering diversity of pricing criteria and the great disparities in profit margins. It analyses the mechanism of the Russian inflation, showing how uncontrolled prices and wages (in theory controlled) rose gradually, with controlled prices following at intervals by large leaps. Some of the resulting administrative difficulties are described.

This is a pleasing book, whose merits include a good understanding of the administrative problems of large scale organisation, clarity of exposition and economy in the selection of information pertinent to the main issues. Although one might occasionally take issue with the details of the theoretical discussion, for instance with some of the statements about the optimal allocations brought about by pricing systems, the conclusions appear to be sound and sensible.

LUCIEN FOLDES.

Productivity and Social Organization: The Ahmedabad Experiment. By A. K. RICE. Tavistock Publications Ltd. 1958. xiii + 298 pp. 35s.

This is an important book. Too often accounts of social research in industry leave the reader wondering whether the conclusions reached are justified by the methods adopted and the facts presented, or, if his point of view is a different one, whether the exercise was worth the money.

In *Productivity and Social Organization,* Mr. A. K. Rice describes in clear and readable language a programme of three years operational research, in which attempts were made to understand and meet the social and psychological problems involved in introducing important technical changes in the Calico and Jubilee Mills of the Ahmedabad Manufacturing and Calico Printing Company in Ahmedabad.

No sweeping claims are made for the research worker's contribution, but as the story of the developments is told, outlining the problems encountered, the hypotheses for possible solutions stated and tested by subsequent events, the relevance of Mr. Rice's work is amply demonstrated.

It is easy to give assent to the idea that a technical change involves social and psychological factors. What has rarely been made so clear before is the importance, in terms of practical management, of studying these elements as part of the total problem of innovation. It was only by the Ahmedabad management's acceptance of the need to bring about a situation that was, so far as possible, socially and psychologically acceptable to all concerned that full advantage of the technical changes was obtained.

The fact that this study was made in a developing rather than in an industrialised area perhaps makes it easier to appreciate the force of Mr. Rice's demonstration, but in no way reduces its significance for highly industrialised countries.

NANCY SEEAR.

Factor Endowment and International Trade. By SUBIMAL MOOKERJEE. Asia Publishing House, Bombay. 1958. viii + 99 pp. Rs.8·75.

In terms of the descriptive note on the cover this book " aims at presenting a systematic statement of the Heckscher-Ohlin Theory ", discusses its application to empirical findings by Leontief and provides its general evaluation. Of the five chapters, four are devoted to the first task and the last one to the remaining two topics. The discussion of the factor-endowment theorem as one of the main causal forces in international trade reproduces the type of analysis that has evolved and become familiar since Samuelson's article of 1949.

Written clearly and without recourse to mathematics the book seems, however, to overstate its case. In particular it fails to stress sufficiently the importance of restrictive assumptions underlying this type of analysis, especially of the technological conditions and variations in the original factor endowment. In the multi-product world the difficulties arising from the condition that the number of goods be at least the same as the number of factors are still greater. And in such a world, as pointed out by Johnson, the concept of factor intensity becomes meaningless. It is on these grounds that the attempt to apply the theory in its primitive form to Leontief's findings appears unconvincing. Nevertheless as an introduction to the subject the book is useful and valuable.

T. M. RYBCZYNSKI.

SHORTER NOTICES

Die Steuern im Vereinigten Königreich. By H. WEISE. Kieler Studien, Kiel. 1957. viii + 360 pp. Dm.40.

This is an exceedingly detailed factual survey of the British tax system. The author begins with a general account of taxation since the first World

War, and then moves on to discuss direct and indirect taxation (including local taxes) in the post-war period. A final part considers the territorial applicability of British taxes, and, in particular, the problem of double taxation. A separate loose-leaf volume contains, in tabular form, summaries of the legal provisions concerning taxation, as well as detailed fiscal statistics.

Financing Free World Trade with the Sino-Soviet Bloc. By RAYMOND F. MIKESELL and JACK N. BEHRMAN. Princeton Studies in International Finance. No. 8. Princeton University. 1958. 101pp. and appendix. 25 cents.

The bulk of this study is devoted to an examination of the details of payment arrangements and trade agreements. A list of these occupies the appendix, which is considerably longer than the rest of the book. This is essentially a work of reference, though there is some brief discussion of policy and some useful consideration of problems set by the eastern habits of bilateral trading. The difficulties of inter-system trade get an all-too-short, but impartial, survey. Thus one can see that Soviet trade negotiators are often irritated by the fact that an undertaking by them to buy can be clearly acted upon, while on the western side everything usually depends on the unpredictable behaviour of private importers.

Analyses of Industrial Operations. Edited by E. H. BOWMAN and R. B. FETTER. Richard D. Irwin Inc., Homewood, Illinois. 1959. viii + 485 pp. $7·95.

The authors are to be congratulated on producing this collection of readings so soon after the appearance of their excellent text on production management to which it is complementary (for a review, see this journal, Aug. 1958, p. 260). Description and discussions of twenty-six applications of modern operations research are included, each paper being preceded by a short summary of its points of special interest prepared by the editors. From the point of view of the general economist, apart from providing further evidence of the relevance of modern mathematical techniques to genuine business problems, this book is of value in that it contains the two important papers by Professor Holt and his colleagues at the Carnegie Institute of Technology on Production and Employment Scheduling, neither of which has hitherto been easily accessible in England.

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